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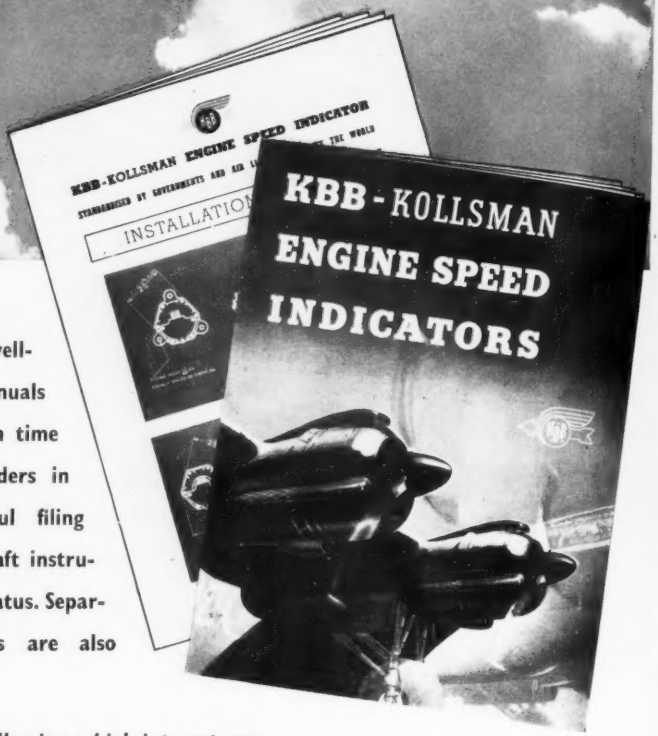

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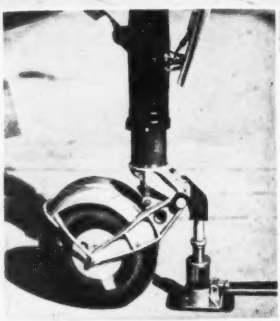

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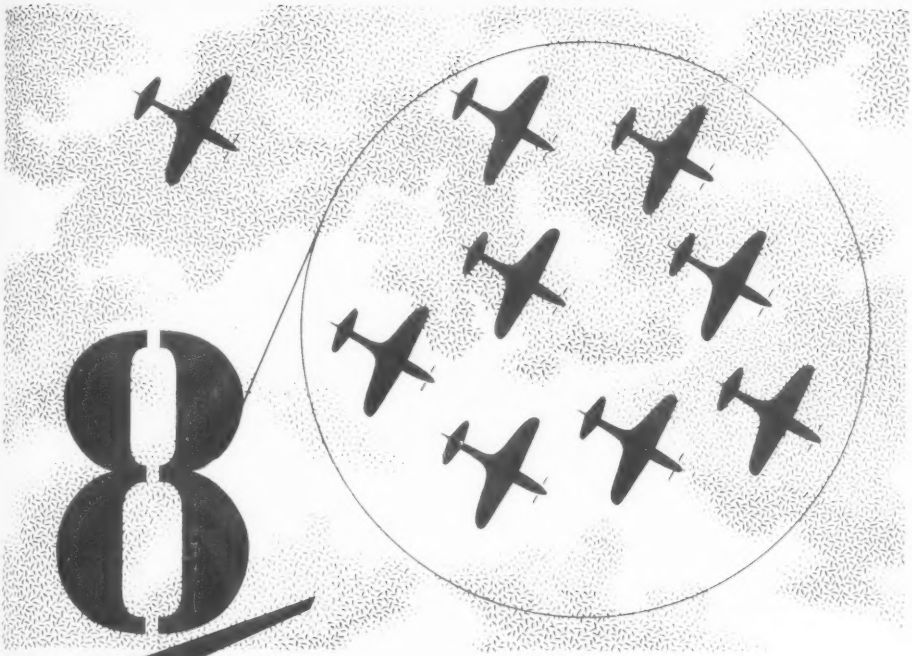

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The 692nd lecture read before the Society.

THE SILENCING OF AIRCRAFT

by

N. FLEMING, M.A.

Mr. Fleming joined the Physics Division of the National Physical Laboratory after taking his degree in Physics at Cambridge in 1923. He has worked on sound measurement and sound-proofing for the past 23 years. He is now head of the Acoustics Section at the National Physical Laboratory.

A MEETING of the Society was held in the lecture hall of the Institution of Civil Engineers, Great George Street, Westminster, London, S.W.1, on Wednesday 10th April 1946, at which a paper on "The Silencing of Aircraft," by Mr. N. Fleming, M.A.. was presented and discussed. In the Chair, Mr. R. S. Stafford, Fellow (Handley Page Ltd.).

The CHAIRMAN conveyed the apologies of the President (Sir Frederick Handley Page) for his unavoidable absence.

Introducing Mr. Fleming, the Chairman said he had obtained his Degree in physics at Cambridge in 1923. He had then joined the National Physical Laboratory, Physics Division, and had worked on sound measurement and sound proofing for the past 23 years.

INTRODUCTION

DURING the war period, effort has been concentrated on the design of military aircraft, in the performance of which operational efficiency has been an overriding consideration. Speeds and powers have increased, resulting in a greater production of noise; but since noise reduction usually involves some loss of performance, perhaps from the additional weight of a sound-proofing treatment, or the slight loss of engine efficiency which may accompany the use of an exhaust collector system, noise has had to be largely ignored. In the military machine, moreover, the incidence of noise on the ears of the crew is mitigated to an appreciable extent by the use of flying helmets incorporating well fitting earphones —a measure which it is hardly practicable to apply to civil machines.

With the return of civil passenger services, employing aircraft of much higher cruising speeds and engine powers than before the war, we are faced with a situation in which the difficulties of providing tolerable noise conditions in the cabin are more severe, while during the intervening period only a little investigatory work has been possible in this country on the reduction of aircraft noise. In the U.S.A. more extended investigations have been made on the efficient design of sound-proofing treatments, but the data remains in confidential documents and is not readily accessible. Work is now being more actively pursued, but there is still much need for the collection of data on

the noises of modern aircraft, and for investigations on methods of noise reduction. More data applicable to the passenger machine of the future, powered by jet engines or gas turbine-driven airscrews, are also required.

The present paper discusses the appropriate methods of noise measurement for such work, and reviews existing knowledge. The results of recent work by the National Physical Laboratory in this field are given.

THE MEASUREMENT OF AIRCRAFT NOISE

1. *General Requirements*

The sensation of sound is produced by the action upon the ear of the small alternating pressure variations in the sound field. Consequently, the physical characteristics of a noise could be sufficiently specified, as regards its action on the ear, by a statement of the variation of the instantaneous sound pressure with time at the point concerned, *i.e.*,by a record of the wave form of the sound pressure. Such a record, however, though valuable for some purposes, is difficult to interpret, and to attempt to compare different noises by this means would be most unsatisfactory. It is necessary, therefore, to seek some simpler method of specifying the characteristics of the noise, which though less complete, may still be adequate for the purpose in view.

In the earlier investigations of noise in aircraft (1), (2) all that was usually attempted was a measurement of the overall physical intensity of the noise or of its loudness. It is now recognised that such measurements alone do not give sufficient information, and that a knowledge of the distribution of the sound intensity throughout the range of audible frequencies is also desirable. The technique of noise measurement, moreover, has progressed simultaneously with the collection of data on aircraft noise. At first there was no general agreement on the units to be used in statements of the intensity or loudness of a noise, and some care is consequently needed in comparing the results of different observers of this time, and in comparing their figures with more recent data. A brief statement of the units now employed, and of the various types of noise measurement which may be appropriate for different purposes, is given below.

2. *Sound Intensity : The Decibel*

The sound intensity in a progressive sound wave is defined as the rate of energy flow per unit area of the wave front. In a plane progressive wave the intensity I is given by

$$I = \overline{p}^{\,2}/\rho c \quad . \quad . \quad (1)$$

where $\overline{p}$ is the root mean square value of the sound pressure
ρ is the density of the medium, and
c is the velocity of sound in the medium.

For air at about 15°C. and normal pressure ρc is approximately 42 in c.g.s. units.

In a more general field of sound, in which waves may be travelling in all directions, no such definition of intensity is possible, but since it is the sound pressure which is concerned in producing the sensation of sound, the intensity at a point is defined by equation (1) $\overline{p}$ then being the root mean square value of the sound pressure at the point concerned.

The range of sound pressures to which the ear responds, from that which is just audible to that which begins to be painful, is from less than 1/1,000 dyne per sq. cm. to the order of 1,000 dynes per sq. cm., corresponding to a range of sound intensities of from about 10^{-16} watts per sq. cm. to 10^{-3} watts per sq. cm. In consequence of this wide range of values and also because change of sensation is more nearly proportional to the fractional increment of intensity than to the absolute increment, it is convenient in expressing sound intensities to use a scale in which the steps correspond to equal

fractional increments, *i.e.*, a logarithmic scale. The one adopted is the decibel scale (3), based upon common logarithms, the proportionate change of intensity from a value I_1, to a value I_2, that is, the ratio I_2/I_1, being defined as a change of $\log_{10}(I_2/I_1)$ bels or $10 \log_{10}(I_2/I_1)$ decibels. Since a number of decibels expresses only a ratio of intensities, it is necessary, in order to define an absolute value, to specify a datum from which the ratio is to be measured. The value now generally employed for this purpose is 10^{-16} watts per sq. cm., corresponding approximately to the threshold of audibility at 1,000 cycles per second, and all intensity levels in decibels, quoted below, are with respect to this datum.

A two-fold change of intensity thus corresponds to 3 decibels, a ten-fold change to 10 decibels, a million-fold change to 60 decibels, etc.: and the whole range of audible intensities, from about 10^{-16} watts per sq. cm. to 10^{-3} watts per sq. cm., is covered in 130 decibel steps.

Measurements of sound intensity are normally made in terms of the related sound pressure, to which most microphones respond. Determination of the overall sound intensity level, for instance, requires a microphone with flat frequency response curve, followed by an amplifier, the gain of which can be adjusted in decibel steps and a rectifier which measures the root mean square value of the amplifier output. Instruments conforming reasonably well to these requirements can readily be made in portable form, and calibrated so as to read directly the sound intensity level in decibels above the prescribed zero.

3. *Loudness: The Phon*

The loudness of a sound—the magnitude of the sensation it produces—is a subjective characteristic, and consequently must be measured primarily by subjective methods. It is obviously related to the physical intensity, since increasing the intensity, without changing the other characteristics, increases the sensation of loudness. Experiments have shown, however, that the relation is complex, and that loudness is a function of the character of the sound as well as of its intensity.

Investigations of the equality of loudness of pure tones (4), (5) have shown, for instance, that for frequencies below 1,000 c.p.s., tones of equal intensity are approximately equally loud at high intensity levels —above about 100 db. As the intensity is reduced, however, the loudness of the lower frequencies falls off more rapidly than that of the higher frequencies. At 50 c.p.s. the threshold of audibility (zero loudness) is reached when the intensity level is still 50 db. above the threshold level at 1,000 c.p.s.

Thus, even for pure tones, neither the absolute physical intensity, nor the intensity relative to the threshold value for the tone concerned, is sufficient to define the loudness, and when complex noises are considered the matter becomes still more complicated.

In consequence, a scale of loudness has been defined based upon subjective estimates. For this purpose a standard comparison sound of adjustable intensity is employed, to which a scale of loudness values is assigned starting at zero at the threshold of audibility and increasing progressively with the intensity. Then the loudness of any noise is defined as equal to the loudness value of the standard sound which the average judgment of a number of observers assesses as being equally loud to the noise. On the phon scale of loudness (3), which was adopted internationally in 1937, the standard sound is a pure tone of frequency 1,000 c.p.s. to which a scale of loudness values—termed phons—is assigned, equal numerically to the intensity level of the tone in decibels above 10^{-16} watts per sq. cm. —the approximate threshold value.

It is to be noted that the steps of the phon

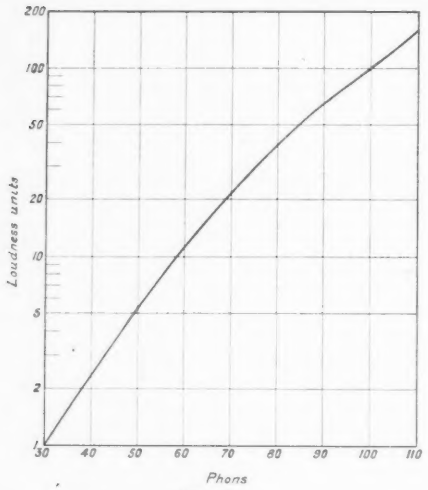

Fig. 1

Relation between phons and a scale of loudness units based on subjective estimates of halving the loudness (Churcher, King and Davies).

scale have been chosen quite arbitrarily as equal to decibel steps of intensity of the standard tone. It cannot be assumed, therefore, that an increase of loudness from 30 to 40 phons produces the same change of sensation as an increase from 100 to 110 phons, nor that the latter change would be considered as a 10 per cent increase of loudness. The interpretation of loudness levels on the phon scale in terms of the sensation produced is a matter for experience, just as is the interpretation of temperature readings in terms of warmth.

Other scales of loudness, designed to avoid some of the disadvantages of the arbitrary nature of the phon scale have been proposed. Churcher, King and Davies (6), for instance, as a result of experiments in which observers were asked to adjust the intensity of a tone so as to halve its loudness, have arrived at a scale of loudness units, the relation between which and the phon scale is shown in Fig. 1. The scale is made to coincide with the phon scale at the values 0 and 100, and it follows from its derivation that halving the number of loudness units corresponds to halving the sensation of loudness. At medium loudness levels (60 phons) it appears that a 10 phon decrease is considered to halve the loudness. At high levels a rather larger decrease in phons is required, and at lower levels a slightly smaller decrease, to halve the loudness.

Instruments for the subjective measurement of loudness in phons are available, in which the standard tone, whose intensity can be varied, in decibel steps, is produced in a telephone receiver. The observer listens to the standard tone in one ear, and simultaneously to the noise in his other ear, and adjusts the level of the standard tone until the two are judged to be equally loud. In another form of instrument, which has been shown to give more consistent results, two earphones are provided, and the standard tone and the noise are observed alternately by placing the telephones on, and removing them from the ears.

Subjective measurements of this nature are never very reliable. The same observer cannot repeat his measurements precisely, and different observers often vary by 10 phons or more in their assessments of equality of loudness, so that a team of observers is needed to obtain a significant mean value. In order to overcome this difficulty attempts have been made to devise objective noise meters which shall give readings in agreement with the average subjective judgment of a number of observers. The American Sound Level Meter (7), for instance, takes some account of the characteristics of the ear by arranging for the sensitivity to be reduced towards the lower frequencies, when the instrument is measuring the lower intensity levels, in accordance with the reduced sensitivity of the ear. It does not, however, profess to measure phons, and indeed may give readings which are very low compared with subjective estimates on noises consisting of a succession of impulses, since it measures the root mean square value of the sound pressure, and the loudness of such noises

is determined more by the peak level of the impulses. Davis (8) has devised a meter which takes this fact into account, and gives satisfactory agreement with subjective determinations for a wide range of noises of moderate loudness levels. King and others (9) have also developed a meter in which both the root mean square value and the peak value are measured, the loudness being determined from the two readings. So far, however, no form of objective noise meter, that is, an instrument intended to give readings in agreement with subjective assessments of loudness, has been considered sufficiently reliable over a wide range of noises to warrant its adoption as a standard.

Although the loudness of a noise is more significant as regards its subjective effect than the overall physical intensity, it is not in itself a sufficient criterion of the nuisance value. For instance, noises of predominantly high pitch tend to be more annoying than equally loud noises in which the low frequencies predominate. Loud noises containing a wide range of frequencies become much less distressing when the high frequencies are suppressed, although the resultant decrease in loudness may be slight. Reduction of the high frequency content also contributes markedly to improvement in the intelligibility of speech in the presence of the noise. These factors are important in the consideration of aircraft noise, and, in

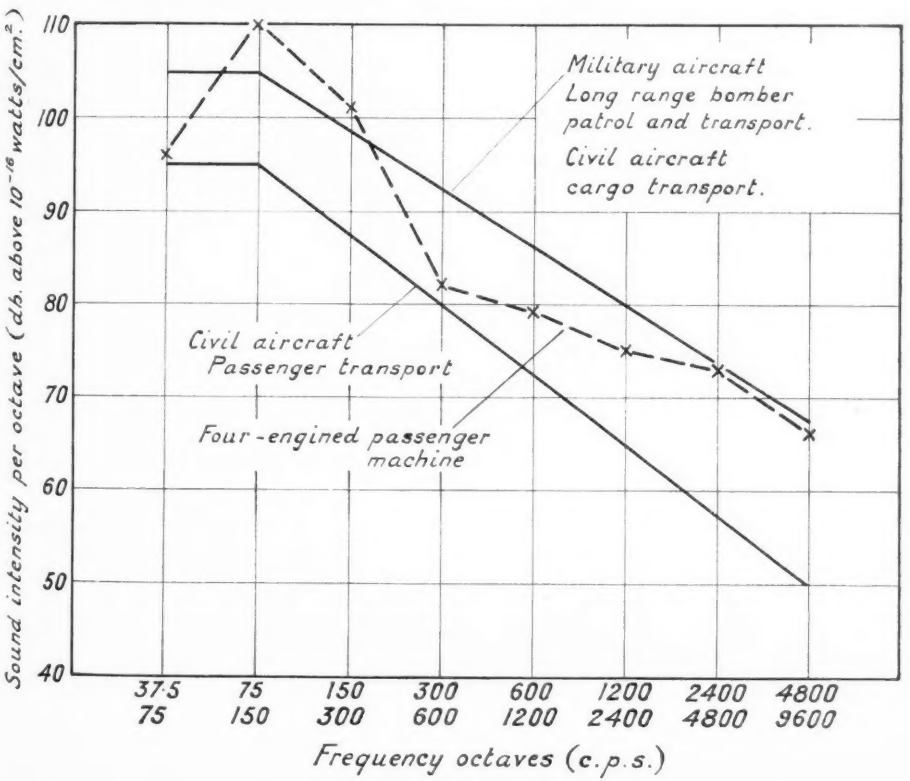

Fig. 2

Recommended maximum intensity levels in terms of sound intensity per octave frequency band, and example of noise analysis in a passenger aeroplane.

consequence, measurements of the noise in aeroplanes should be designed to show the distribution of the sound intensity with frequency. For this purpose some form of noise analysis is required.

4. *Noise Analysis*

The distribution of sound energy with frequency is determined experimentally by introducing into the measuring equipment electrical filter circuits designed to pass successive bands of frequencies of appropriate width. For a general description of aircraft noise a comparatively coarse analysis is sufficient and a band width of one octave is chosen, the frequency range from about 40 to 10,000 c.p.s. being covered in eight such bands. The apparatus required is comparatively simple, and readily portable, involving only the addition of the electrical filter circuits to the equipment for measuring the overall intensity.

For some purposes a finer analysis is desirable, as, for instance, when the noise contains pronounced single frequency components, and it is desired to examine these in detail. Such components occur in aircraft noise at the lower frequencies, arising from the airscrew and engine. A band width of 10 or 20 cycles would then be more suitable, and would enable the components produced by the airscrew and those produced by the engine to be identified separately, and the relative importance of these two sources in contributing to the noise to be determined.

On the basis of the analysis of the noise into octave frequency bands, the Royal Aircraft Establishment has made recommendations as to the maximum levels which should be permitted in aircraft of different types (10). These recommended levels are shown in Fig. 2 For military aircraft and civil cargo transport the levels vary from 105 db. in the two lowest octaves (37.5 to 150 c.p.s.) to 68 db. in the highest octave (4,800 to 9,600 c.p.s.). For civil passenger machines the recommended maximum levels are from 10 db. lower at the low frequencies to 18 db. lower at the high frequencies. The Figure also includes an example of a noise analysis in a four-engined passenger machine with sound-proofing treatment. The noise levels here were considerably above the recommended maxima. The recommendations are based upon what it is considered can be achieved reasonably, as well as upon considerations of comfort. It should be borne in mind that they are maximum levels and are not to be considered as levels below which further reduction is of no benefit. On the contrary, any further reduction which should prove to be practicable would be well worth while.

REDUCTION OF NOISE AT THE SOURCE

1. *General Considerations*

The reduction of the noise in the cabin of an aeroplane requires either a sound-insulation treatment to the cabin walls, etc., or the reduction of the noise at its source or sources, or both. The main sources of noise are the engine, the airscrew and aerodynamic noise produced by the flow of air over the structure. In considering the reduction of noise at the source it is important to bear in mind that the source of highest intensity must be attacked first. For instance, if the intensity of noise from one source is 6 db. higher than that from a second, the reduction in the total noise which would be obtained by completely suppressing the smaller source would be only 1 db. There is little to be gained, therefore, by reducing the noise from one source to a lower level than about 6 db. below that of the noise from the remaining sources.

It is important, therefore, to know the relative contributions of the various sources of aircraft noise to the total noise intensity. The relative order of importance may of course be different at different parts of the frequency range. Ten years or so ago, as judged by measurements of the overall loudness, the airscrew was the predominant

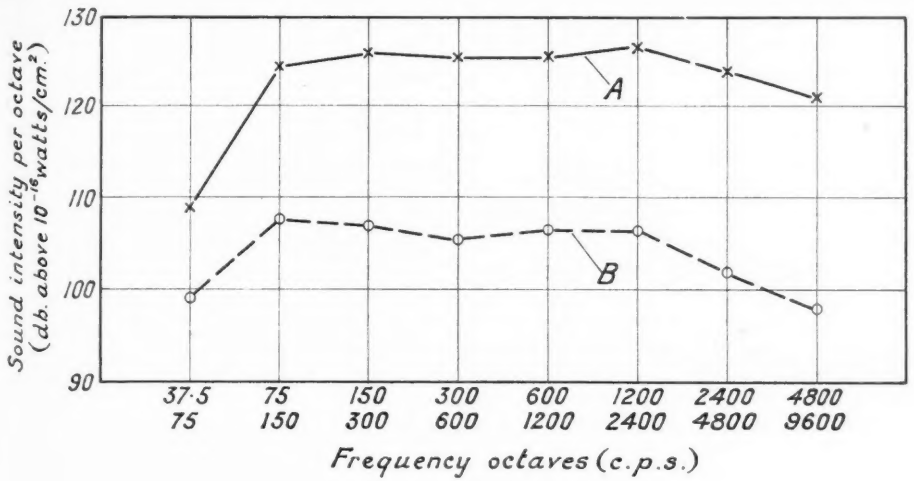

Fig. 3
Noise analyses of engines on the test bed.
A—with open stub exhausts
B—with exhaust manifolds connected to silencing system of test bed.

source, at any rate with tip speeds of the order of 700 to 800ft. per sec. With tip speeds of 500ft. per sec. the engine exhaust noise was the main source. When this noise was reduced to below that of the airscrews by the use of exhaust silencers, it was found that there was practically no difference in the noise levels in level flight under power and in gliding flight at the same air speed with engines throttled back, indicating that aerodynamic noise was then the greatest source.

In modern passenger aircraft the data available are not yet sufficient to show conclusively which source is the most important in each of the successive octave frequency bands. With ejector type exhausts, the engine exhaust noise appears to be the most predominant at low frequencies, the highest noise levels occurring in the octave band from 75 to 150 c.p.s., which normally includes the explosion frequency from the exhaust stubs on one side of the engine. At the high frequencies above about 600 c.p.s. there is some evidence that at speeds of the order of 200 m.p.h. aerodynamic noise may be the most important.

2. *Engine Noise*

Piston Engines.—The predominant noise from a piston-type engine is that from the exhaust. To the side of the engine, the noise at low frequencies consists mainly of the explosion frequency from the exhaust stubs or manifold on that side, and its harmonics. At high frequencies discrete components are not apparent and the sound is more uniformly distributed throughout the frequency range. The maximum intensity per octave is usually in the band containing the fundamental of the explosion frequency, *i.e*, with a four-stroke engine with six cylinders discharging to one side, in the octave band from 75 to 150 c.p.s., for engine speeds of from 1,500 to 3,000 r.p.m. This is not always the case, however, and there may be little difference between the intensity in this band and in each of the higher bands.

As an example of the degree of noise reduction which may be expected from suppression of the exhaust noise, analyses are shown in Fig. 3 of the noise from two engines of the same type (liquid-cooled) under the same operating conditions—2,400 r.p.m. and 7lb. boost. Both the engines

were on dynamometer test beds of the same type, and the measurements in each case were made at about 8ft. to the side of the engine. Curve *A* is for an engine with open stub exhausts, and curve *B* for an engine with exhaust manifolds connected to the exhaust silencing system of the test bed. The difference is of the order of 20 db. except in the lowest frequency band. With the engine enclosed in a nacelle, some of the general clatter of the engine might also be suppressed, and the improvement might be greater.

The manner in which the noise may vary with the operating conditions is illustrated in Fig. 4. These curves are again for a liquid-cooled engine with stub exhausts on a dynamometer test bed, the analyses being made at a point about 8ft. to the side of the engine. Curves *A* are for constant speed and varying horse power, and curves *B* for constant horse power and varying speed.

The test bed was open at the two ends but enclosed at the sides and above, and in interpreting the results of noise analyses made in such a situation the effect of the enclosure must be considered. At low frequencies, where the noise consists mainly of discrete components of the frequency of the explosions and its harmonics, reflection from the walls will interfere with the direct sound, giving positions of high or low intensity according to the phase relation between the direct sound and the reflected sound. This interference system will shift as the engine speed varies, so that a position of high intensity for the fundamental of the explosion frequency at one engine speed may be a position of low intensity at another speed. Consequently, at low frequencies, variations in the engine noise with varying engine speed may be obscured by this effect. At higher frequencies, the effect of interference tends to be averaged out, both because the number of harmonic components in the octave band increases, and because the noise consists less of discrete components. At high frequencies the effect of the enclosure is thus largely independent of the particular position of measurement, and is simply to increase the intensity by an amount of the order of perhaps 5 db. above what it would be in the open.

The curves A of Fig. 4, therefore, for which the engine speed was constant, give a true indication of the variation of the noise with horse power. The same general effects were obtained with a number of different stub and manifold exhaust systems. At low frequencies, in the bands up to that containing the explosion frequency (132 c.p.s.), there is little variation of noise with horse power, and for clarity only the lowest and highest readings are shown in the Figure. On the average, for the different exhaust systems tested, the noise intensity in this frequency region increased with horse power, but less rapidly than in direct proportion, *i.e.*, by less than 3 db. for a two-fold increase of horse power. In the higher frequency bands the noise level increases more rapidly with horse power. For the three highest bands, *i.e.*, for frequencies above about 1,200 c.p.s., the increase was on the average about 8 db. for a two-fold increase of horse power.

At constant horse power and varying speed (curves B of Fig. 4), the high frequency noise remains almost constant, and as before only the highest and lowest readings are shown. On the average, for frequencies above 1,200 c.p.s., there was no increase in noise for engine speeds from 1,800 to 2,600 r.p.m., and an increase of about 1.5 db. at 3,000 r.p.m. At the low frequencies the variation with engine speed is irregular. This is due, in part at any rate, to the effect of interference, and no reliable conclusions as to the actual variation of the total noise output can be drawn. In the octave band from 300 to 600 c.p.s., which includes three or four harmonics of the explosion frequency, the level increases regularly with engine speed, and this was generally the case for six different exhaust systems tested. In

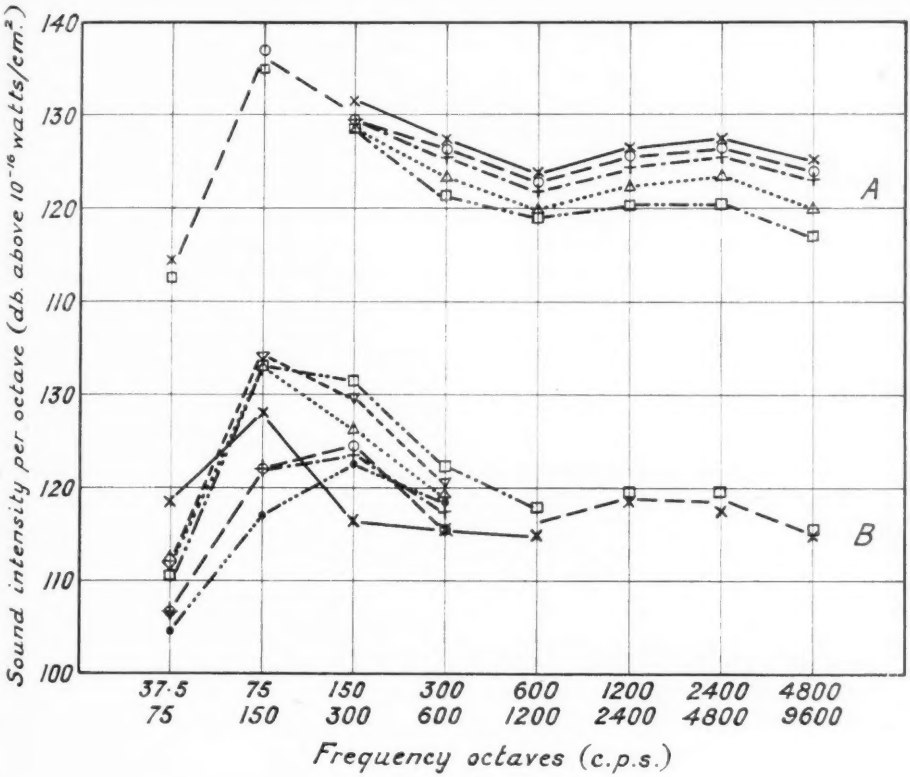

Fig. 4

Variation of engine noise with operating conditions

A—constant speed (2,650 r.p.m.) and varying power

□ 660 B.H.P. △ 780 B.H.P. + 890 B.H.P.
○ 1,000 B.H.P. × 1,120 B.H.P.

B—constant power (600 B.H.P.) and varying speed

× 1,800 r.p.m. ○ 2,000 r.p.m. + 2,200 r.p.m.
● 2,400 r.p.m. △ 2,600 r.p.m. ▽ 2,800 r.p.m.
□ 3,000 r.p.m.

each case the rate of increase with engine speed was roughly the same, being on the average about 8 db. for a two-fold increase of speed.

Before the war exhaust silencers of different types were designed which reduced the exhaust noise by amounts of from 15 to 20 db. in the octave band from 75 to 150 c.p.s. up to 20 to 30 db. in bands above 600 c.p.s. In use on aircraft at that time they reduced the engine noise to below the level of the airscrew noise, even with low tip speeds of the order of 500ft. per sec. Owing, however, to their weight, the loss of engine efficiency they may produce, and the increased fire risk they entail, there is little prospect of silencers of this type being acceptable for modern aircraft.

A considerable reduction of noise, however, can be obtained by the use of exhaust manifolds instead of separate stubs, and there is also considerable variation in the exhaust noise from the same engine with different types of stubs. This is illustrated

by Fig. 5, which shows analyses of the noise from the same engine with three different types of stubs and three different types of manifold, for each of two conditions of operation. Noise reduction may of course be accompanied by loss of engine power and of exhaust thrust. The two, however, do not necessarily go hand in hand, and development in this direction is required to determine the most effective type of exhaust system to give the greatest noise reduction for the smallest loss of efficiency.

Jet Engines.—In the jet engine the main sources of noise are the whistle from the impellor, and the roar of the jet. No information is available yet on the noise of such engines in flight and any comparison of the noise with that of piston engines must be based on measurements made on the test bed.

The frequency of the whistle from the impellor is determined by the number of blades on the impellor and its rotational speed. On one type of engine, where this frequency was of the order of 2,500 to 3,000 c.p.s. the intensity in the octave band containing this

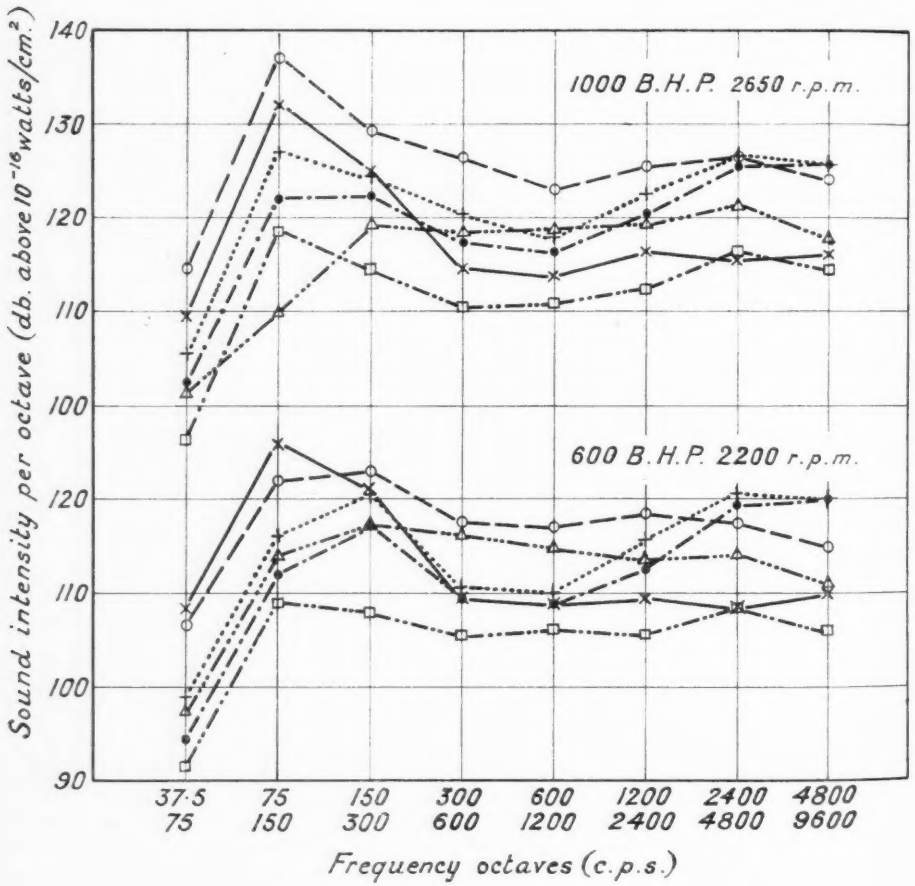

Fig. 5

Variation of engine noise with type of exhaust system

○ + ● — separate stubs

× □ △ — manifolds

component, measured in the filter room of the test bed immediately in front of the intake, was about 115 db. under normal cruising conditions, and increased by 5 db. when the engine speed was increased by 15 per cent. With a different engine in which the frequency of the impellor whistle was of the order of 10,000 c.p.s. the level a few feet from the front of the engine on an enclosed test bed was 120 to 125 db. It is to be noted that the ear is some 15 db. less sensitive to a frequency of 10,000 c.p.s. than to a frequency of 3,000 c.p.s., so that from the noise point of view there is an advantage in having the higher frequency of impellor noise.

The noise from the jet is considerably greater at an angle than near to the axis of the jet. Estimates of the noise level at 10ft. from the jet orifice, at an angle of 45° to the jet, made from measurements at greater distances, show that, for engines developing about 3,000 lb. thrust on the test bed, the maximum levels usually occur in the two octave bands from 75 to 300 c.p.s., and are of the order of 120 to 130 db. At high frequencies the levels fall to about 115 db.

Thus, at the low frequencies, the noise from the jet is of the same order as that from a piston engine with stub exhausts developing 1,000 horse-power, and at the high frequencies it is appreciably less. With a propeller efficiency of 80 per cent., and an air speed of 200 m.p.h., the corresponding thrust obtained from the piston engine would be only about 1,500 lb. In flight, moreover, the noise from the jet at the same thrust would probably be rather less owing to the lower velocity of the jet relative to the surrounding air. On the whole, therefore, there are grounds for believing that, for the same performance, an aircraft powered with jet engines may be less noisy than one powered with piston engines with open stub exhausts. There is, however, some prospect of being able to reduce the exhaust noise of the piston engine by the use of manifolds—a course which may be necessary in order to achieve tolerable conditions—whereas the jet engine presents no such opportunity. No firm conclusions can yet be drawn, and noise measurements in jet propelled aircraft are urgently required, so that, if necessary, attention can be given in design to the most favourable lay-out of the passenger accommodation with respect to the engines.

3. *Airscrew Noise*

Various theories of the production of noise by a rotating airscrew have been given. Gutin (11), for instance, calculates the noise produced by the forces exerted on the air by each element of the blades, *i.e.*, by the torque and thrust of each element. This portion of the noise consists of a series of harmonic components, the fundamental of which is equal to the rotational speed times the number of blades, and Deming (12) has found reasonable agreement between the theory and experiment for the first four harmonics. At low tip-speeds the fundamental is the predominant component, but with increasing tip-speed the sound power of the successive harmonics increases more rapidly the higher the order of the harmonic, and at high tip-speeds the harmonics may become the most important. For the particular airscrew considered by Deming, for instance, the power in the fundamental increased as the fourth power of the tip-speed and that of the fourth harmonic as the ninth power of the tip-speed. The fundamental component has a maximum intensity in a direction about 30° behind the plane of the airscrew disc, and the directions of maximum intensity of the harmonics lie successively nearer to this plane.

In addition to this torque and thrust noise, there is noise due to the shedding of vortices from the blades, which may be the more important at high frequencies, and when the tip-speed approaches the velocity of sound shock waves are produced, giving a large increase in the high frequency content of the noise.

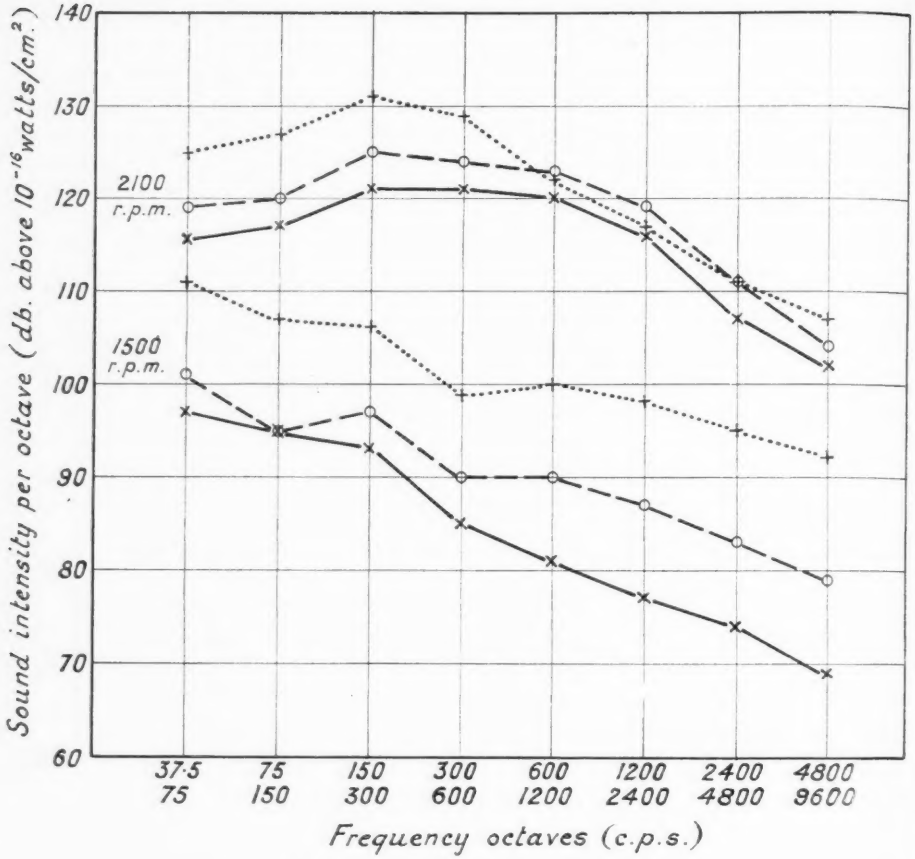

Fig. 6

Examples of octave analyses of airscrew noise. Two blade airscrew, 9 ft. 6 in. diameter, zero forward speed. Measurements at 35 ft. in the plane of the airscrew. Blade angle (at 0.75 radius)

+ 20°, ○ 11°40′, × 7°40′.

Measurement of airscrew noise in terms of the individual harmonic components is difficult, requiring a site free from surrounding buildings in order to avoid interference from reflections. Certain broad correlations between noise and operating conditions can be obtained, however, from octave analyses of the noise under less stringent experimental conditions.

Fig. 6 shows octave analyses of the noise from a two-bladed airscrew of 9 ft. 6 in. diameter on the spinning plant at the Royal Aircraft Establishment. The site here has various small buildings at the sides at distances of 30 to 40 ft., and larger buildings at greater distances on all sides. The measurements shown were made at a distance of 35 ft. from the airscrew hub, in the plane of the disc, and refer to three different blade angles at each of two speeds. Similar measurements were made at two intervening speeds, and at three other positions. It is obvious from these curves that the variation of the noise with operating conditions is different for different parts of the frequency range. To examine the variation, therefore,

the total intensities in the lowest three bands (37.5 to 300 c.p.s.) and in the highest three bands (1,200 to 9,600 c.p.s.) were considered separately, and a rough value of the mean intensity in all directions was calculated from the results for the four different measurement positions.

These values are shown plotted against the logarithm of the ratio of the tip-speed to the velocity of sound (V_0/C) in Fig. 7A. At the lower frequencies there is an approximately linear relation over the whole range of tip-speeds from 750 ft. per sec. to 1,050 ft. per sec. The slope of the lines drawn is the same for each blade angle and corresponds to sound intensity proportional to the 13th power of the tip-speed at constant blade angle. In the range considered this is equivalent to an increase of about 7 db. for each 100 ft. per sec. increase of tip-speed. At the high frequencies there is again an approximately linear relation, but at tip-speeds up to about 950 ft. per sec. only ($V_0/c = 0.85$). In this range the rate of increase with tip-speed is smaller than at the low frequencies, the slope of the lines drawn corresponding to sound intensity proportional to the 8th power of the tip-speed for constant blade angle, but the increase with blade angle is more rapid than for the low frequencies. At tip-speeds above about 950 ft. per sec. there is a rapid rise in the high frequency content of the noise, due no doubt to the formation of shock waves.

In Fig. 7B the same data are shown plotted against the logarithm of the horse-power absorbed by the airscrew, with tip-speed as parameter. At the low frequencies there is again an approximately linear relation, the slope of the lines drawn corresponding to sound intensity proportional to horse power to the power 5/3 at constant tip-speed. The increase with tip-speed at constant horse power also gives a linear relation on a logarithmic basis, with slope corresponding to approximately the 8th power of the tip speed. Thus for the low frequencies the sound intensity is proportional to $(\text{H.P.})^{5/3}$ $(V_0/C)^8$, for all blade angles within the range considered. At the high frequencies, up to tip-speeds at which shock waves begin to be produced, the points corresponding to the different tip-speeds all lie on the same line, and the sound intensity depends solely on the horse power, and is independent of the particular blade angle and speed at which that horse power is absorbed. The slope of the line corresponds to sound intensity proportional to $(\text{H.P.})^{8/3}$. The degree to which these relations accord with the measured values is shown in Fig. 7C, where the mean value of the sound intensity at 35 ft. (in decibels above 10^{-16} watts per sq. cm.) is plotted against

$$10 \log_{10} \{ (\text{H.P.})^{5/3} (V_0/C)^8 \} + 81.5$$

for the low frequencies, and

$$10 \log_{10} (\text{H.P.})^{8/3} + 31$$

for the high frequencies.

Similar correlation between noise and operating conditions, though not quite so good, was obtained for a five-bladed airscrew of diameter 14 ft., for a range of blade angles (at 0.75 radius) from 8°30′ to 17°30′, and a range of tip-speeds from 660 to 1,100 ft. per sec. At the low frequencies the noise intensity was again proportional to about the 5/3 power of the horse power at constant tip-speed. At constant horse power the noise increased more rapidly with tip-speed than with the two-bladed airscrew, being proportional to approximately the 10th power of the tip-speed. The mean sound intensity (in decibels above 10^{-16} watts per sq. cm.) in the three lower octave bands, at a distance of 35 ft., was given approximately by

$$10 \log_{10} \{ (\text{H.P.})^{5/3} (V_0/C)^{10} \} + 77.,$$

At the high frequencies, up to tip-speeds of 950 ft. per sec. the corresponding expression was $10 \log_{10} (\text{H.P.})^{8/3} + 21$. At higher tip-speeds the high frequency noise again increased rapidly above the value given by this relation, and was no longer dependent solely on the horse power.

At the same horse-power, and for similar blade angles the tip-speed of the five-bladed airscrew is very much less than that of the

two-bladed airscrew, with the result that the low frequency noise is much reduced. At 500 horse power and blade angles of about 12°, the corresponding tip-speeds were about 1,050 ft. per sec, and 650 ft. per sec. respectively, and the noise of the five-bladed air-

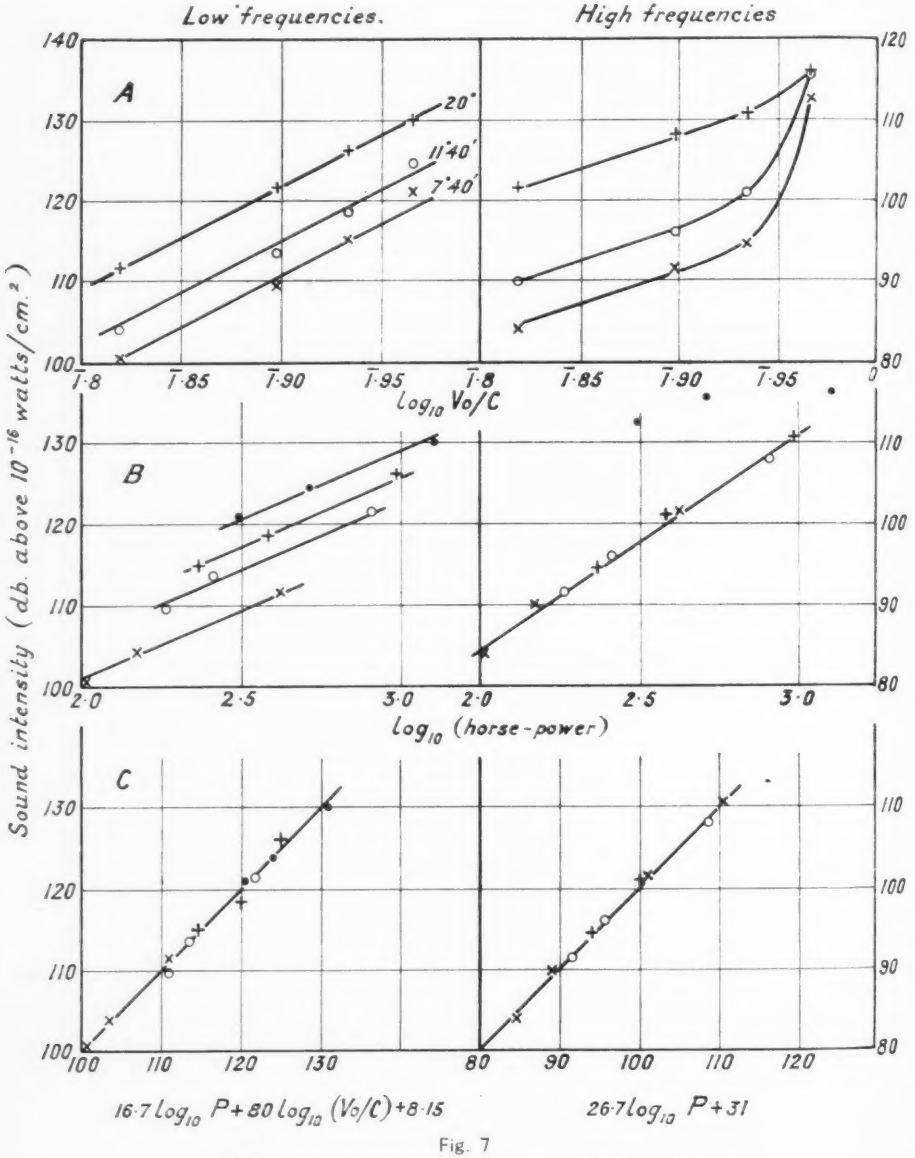

Fig. 7

Variation of airscrew noise with operating conditions

A—variation with tip speed (V_o) at constant blade angle
B—variation with horse-power at constant tip speed
C—agreement of measured values with empirical relations

screw was more than 20 db. lower than that of the two-bladed airscrew. At high frequencies, for corresponding horse powers the noise from the former airscrew was about 10 db. below that from the latter.

The above results are for zero forward speed and require extending to cover the effect of this factor. This could probably be satisfactorily accomplished by measurements in an open jet wind tunnel. In view of the comparatively simple correlations obtained between noise levels and operating conditions, the investigation of a larger number of airscrews in the same manner might enable the effects of variation of other parameters, such as diameter, number of blades and blade shape to be assessed.

It is obvious, however, as has long been recognised, that reduction of airscrew noise requires primarily a reduction of tip-speed. for the same performance this involves either increasing the number of blades or increasing the diameter. Both these measures have disadvantages—increase of weight and increase of moment of inertia. The latter measure may also involve the provision of larger ground clearances and, in multiple engined machines, increased distance of the engines from the centre line of the aircraft. It is for the designer to weigh these disadvantages against the need for smaller noise production.

4. *Aerodynamic Noise*

Little information is available on the magnitude of the noise produced by the flow of air over the structure of the aircraft. In 1934 Morley (13) showed that in an aircraft with silenced exhausts and airscrew of low tip-speed, the overall loudness of the noise was the same in level flight under power and in gliding flight with engines throttled back, at speeds giving the same total velocity of air over the fuselage. The glides were made at speeds of from about 100 to 150 m.p.h. and the overall loudness increased by about 10 phons for this change of air speed.

Recent noise analyses, made in a passenger machine with exhaust manifolds, showed that at a speed of 200 m.p.h. there was no difference between the noise in level flight under power, and in gliding flight with engines stopped and airscrews feathered, at frequencies above 600 c.p.s At these frequencies, moreover, the noise levels were appreciably greater than the maximum recommended levels (Fig. 2).

Thus, even at speeds of 200 m.p.h. or less, aerodynamic noise may predominate, at any rate at the higher frequencies, and with increasing air speeds it is likely to become still more serious. The investigation of this source of noise to determine the manner in which it varies with air-speed, how it is affected by small surface irregularities, such as may occur at window mountings, or by the fairing of the pilots' windows, may present considerable difficulties. It is a problem, however, which it will be increasingly necessary to tackle as the speed of passenger aircraft increases.

SOUND-PROOFING.

1. *Theoretical*

In discussing sound-proofing treatments it will be necessary to make use of the following terms:—

Sound transmission coefficient (t)—the ratio of the sound energy transmitted through a structure to that incident upon it.

Sound reduction factor or transmission loss in decibels (r)—the ratio in decibels of the sound energy incident upon a structure to that transmitted through it, i.e. $10 \log_{10} (1/t)$.

Sound absorption coefficient (α)—the ratio of the sound energy absorbed at a surface to that incident upon it.

The sound reduction between the outside and the inside of an enclosure depends both upon the sound insulation of the walls, and upon the degree of absorption within the enclosure. This may readily be seen by considering the extreme case of transmission of sound into an enclosure in which there is

no absorption. Then the sound energy entering the enclosure through the walls builds up through repeated reflections inside, until a state is reached when the rate at which sound energy enters the enclosure is equal to the rate at which it is transmitted out again, i.e., until the intensity inside is the same as that outside. Thus, however high the transmission loss through the walls may be, there is no reduction between the sound levels outside and inside the enclosure. This extreme case cannot of course occur in practice, but the efficacy of a sound insulating treatment may be considerably reduced if adequate sound absorption is not provided.

An estimate of the relative importance of the two factors may be made if it is assumed that the sound incident on the outside of the enclosure has the same intensity at all points and is randomly directed. Consider an enclosure made up of a number of portions having areas S_1, S_2 etc., with sound transmission coefficients t_1, t_2 etc., and sound absorption coefficients at the inner surface α_1, α_2 etc. Suppose also that there is within the enclosure an additional absorption A*, not distributed on the walls but due to passengers, seats, etc. If the average sound intensity outside the enclosure is I_1, then the intensity (I_2) inside is given by

$$I_1/I_2 = 1 + (\Sigma\alpha S + A)/\Sigma tS \quad \ldots\ldots\ldots(2)$$

In the case of the aircraft cabin we may make the reasonable assumption that the sound transmitted through the floor is negligible compared with that transmitted by the walls, and that the latter, to the whole area of which a sound insulating treatment can be applied, are of uniform construction.

For the untreated cabin, equation (2) then gives for the sound reduction in decibels (R)

$$R_0 = r_0 + 10 \log_{10} (t_0 + \alpha_0 + A/S) \quad \ldots\ldots\ldots(3)$$

where S is the total area of the walls, and the subscript O refers to the untreated walls. It is to be noted that in practice the second term on the right of equation (3) has a negative value, so that the actual sound reduction obtained (R_0) is less than the sound reduction factor of the walls (r_0).

If now a sound insulation treatment is applied to the walls the sound reduction between the outside and inside becomes

$$R_1 = r_1 + 10 \log_{10} (t_1 + \alpha_1 + A/S) \quad \ldots\ldots\ldots(4)$$

The improvement afforded by the treatment is given by

$$R_1 - R_0 = (r_1 - r_0) + 10 \log_{10} (t_1 + \alpha_1 + A/S)/(t_0 + \alpha_0 + A/S) \quad \ldots\ldots\ldots(5)$$

The first term of equation (5) is the improvement in the sound reduction factor of the walls and the second term represents the effect of the added absorption. Except at low frequencies t_1 and t_0 are small compared with A/S and the second term may then be written

$$10 \log_{10} (\alpha_1 S + A)/(\alpha_0 S + A)$$
$$= 10 \log_{10} \{ \text{total absorption after treatment}/\text{total absorption before treatment} \}$$

Thus the improvement due to the absorption of the treatment depends upon the degree of absorption already provided by the seats, passengers, etc. Specific test data for the absorption of aircraft seats are not available, but some estimate of the value of A/S may be made from data for theatre seats and their occupants. Consideration of a cabin, with carpeted floor and seats well upholstered in fabric, in which the floor area per seat is about 10 sq. ft., and the wall and ceiling area about 30 sq. ft., leads to probable values of A/S ranging from about 0.15 at about 100 c.p.s. to 0.30 at 4,000 c.p.s.

In order to illustrate the relative contributions of the sound insulation and the sound absorption of a good sound-proofing treatment to the walls of such a cabin some experimental values of the factors involved in equation (5) are given in Table 1, together with the improvements in the noise level calculated from that equation.

It will be observed that the effect of the added sound absorption is valuable at all frequencies but is particularly so at the low frequencies where the improvement in the sound reduction factor is small.

* A is the area of surface of unit absorption coefficient of equivalent effect.

TABLE I

RELATIVE CONTRIBUTIONS OF THE SOUND INSULATION AND THE SOUND ABSORPTION OF A SOUND-PROOFING TREATMENT TO NOISE REDUCTION IN A FURNISHED PASSENGER CABIN

	Frequency range (c.p.s.)			
	100 to 200	300 to 700	1,000 to 2,000	3,000 to 4,000
t_o	.12	.05	.006	.003
t_1	.065	.004	.00004	.000002
α_o	.07	.13	.05	.05
α_1	.77	.70	.52	.50
A/S	.15	.20	.25	.30
Improvement due to absorption	5 db.	4 db.	4 db.	3 db.
Improvement due to insulation $(r_1 - r_o)$	3 db.	11 db.	22 db.	32 db.

2. *Sound Insulation*

The general principles to be followed in the design of light structures to give high sound insulation are well known, and are illustrated by the curves of Fig. 8, which show the general trend of the variation of the sound reduction factor with weight in different frequency ranges, for different types of construction. The curves are based upon measurements made at the N.P.L. in 1941.

The sound insulation of a panel normally increases with the frequency, though not in a regular manner, particularly at the low frequencies where the effect of resonances of the panel are most pronounced. Since these resonances may depend on the particular size of a panel tested, the irregularities have been smoothed out in Fig. 8 by averaging the sound reduction factors over three frequency ranges—50 to 300, 500 to 1,000 and 1,600 to 4,000 c.p.s.

For single, solid, non-porous panels it is then found that the insulation is determined almost entirely by the superficial weight, and is very little affected by the nature of the material. The general trend of variation of the sound reduction factor with weight for such panels is shown by the full lines. Better sound insulation for a given weight may be obtained at the higher frequencies by the use of double constructions. The dashed curves show the effect of adding non-porous inner linings of various weights to a panel of aircraft fuselage construction with a skin of weight about 0.5 lb. per sq. ft. The spacing between the inner skin and the lining varied from about 1 in. along a central rib of the panel to 2 in. at the sides. It will be noted that for a given total weight there is little improvement over a single solid panel at frequencies up to 300 c.p.s., but considerable improvements are obtained at the higher frequencies. At some low frequencies indeed the insulation of the double partition may be rather less than that of a single partition of the same total weight.

The insulation provided by double partitions also increases with the spacing of the components. This is illustrated by some experiments made at the same time on double windows. Table 2 shows the improvement in insulation of double windows of two sheets of 3/32 inch Triplex, over that of a single window of 3/16 in. Triplex at various spacings of the two components.

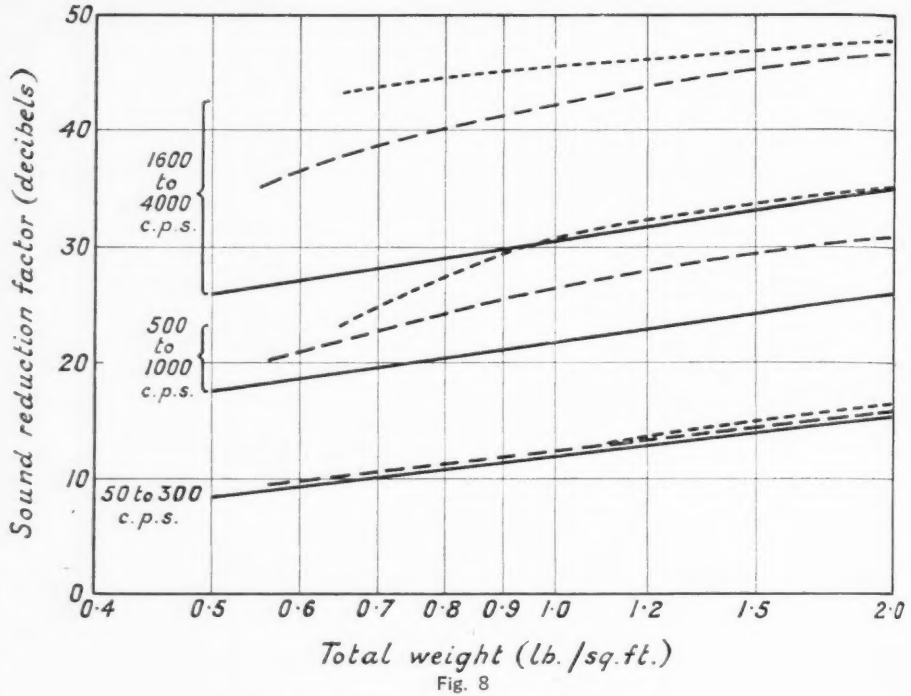

Fig. 8

Variation of sound insulation with weight

Full lines — single skin
Dashed lines — outer skin of weight 0.5 lb. per sq. ft. plus non-porous inner skin spaced one to two inches away
Dotted lines — ditto, plus Kapok, 0.13 lb. per sq. ft., between skins.

It is to be noted that the dashed curves of Fig. 8 refer to non-porous inner linings. A porous lining is not nearly so effective. Thus a sheet of woolfelt of weight 0.25 lb. per sq. ft., tested on the same panel, gave improvements over the untreated panel of 2, 3 and 7 db. in the three frequency ranges respectively, compared with 2, 6 and 13 for a non-porous lining of the same weight.

Still better insulation for a given weight is obtained by inserting a layer of light sound-absorbent material in the space between the outer skin and the lining. The dotted curves of Fig. 8 show the effect of

TABLE II

EFFECT OF SPACING OF COMPONENTS ON THE INSULATION OF DOUBLE WINDOWS OF 3/32IN. PERSPEX

Spacing of components	Improvement of insulation over that of single window of same total weight (decibels)			
	50 to 100 c.p.s.	150 to 300 c.p.s.	500 to 1,000 c.p.s.	1,600 to 3,000 c.p.s.
¼ in.	0	−3	1	14
½ in.	−1	−4	6	15
1 in.	−2	−3	11	15
2 in.	−1	1	14	17

introducing a layer of Kapok enclosed in thin muslin. The weight was 0.13 lb. per sq. ft. and the Kapok was in contact with the outer skin only. There is again little improvement at the low frequencies but considerable improvement at the high frequencies. With a greater weight of Kapok, giving a thickness sufficient for it to press against both the skin and the inner lining, the insulation was considerably reduced in the middle frequency range.

A similar effect can be obtained with double windows by lining the reveal between the two components with sound absorbent material. The improvement in insulation so obtained is inappreciable at frequencies below 1,000 c.p.s., and increases from about 1 db. at 1,000 c.p.s. to the order of 8 db., at 4,000 c.p.s.

Of the absorbent materials tested at that time, which included various wool felts, and asbestos and other mineral and vegetable fibre felts, Kapok proved to be the most effective for its weight, though flame-proofed cotton wadding was equally good.

A construction of the type considered, in which the inner skin is of a non-porous nature, may provide appreciable absorption at its inner surface at the lower frequencies when a light skin such as leather cloth is used, but will give little absorption at the high frequencies. Consequently the additional noise reduction which can be obtained from the absorption at the inner surface of a treatment will be absent at high frequencies. To obtain high absorption at the high frequencies requires the use of a porous inner trim, but the substitution of this for a non-porous lining would result in loss of insulation. The most effective form of treatment, therefore, consists of first a layer of light sound absorbent material, then a non-porous membrane followed by a second layer of sound absorbent and finished with a porous trim.

More recently further measurements have been made on a number of practical sound insulating treatments to a panel of aircraft fuselage construction of dimensions about 10 ft. wide by 8 ft. high. The panel was curved in the vertical direction with a radius of 5 ft. 6 in., and was constructed of vertical frame members 3 in. deep, with horizontal stringers $\frac{3}{4}$ in. deep, and a skin of 20 G. alloy of weight about 0.52 lb. per sq. ft. The sound insulation and the sound absorption of a number of sound-proofing treatments to this panel were measured. The materials employed, together with their superficial weights are given in Table 3.

TABLE III

MATERIALS EMPLOYED IN SOUND-PROOFING TREATMENTS

Material	Superficial weight (lb. per sq. ft.)
Trim cloths	
Vynide (type of leather cloth)	0.099
Leather	0.134
Wool cloth	0.149
Absorbents	
Fiberglas, $\frac{1}{2}$ in. thick	0.027
do. + V film* on one side and muslin on other side	0.043
do. + V film on one side and calico on other side	0.069
do. + calico on one side	0.057
Kapok quilted between muslin, $\frac{1}{2}$ in. thick	0.061
do. + V film on one side	0.072
Stonefelt, $\frac{1}{2}$ in. thick	0.184
do. + calico on each side	0.250
Woolfelt	0.118

* Thin rubber film to prevent moisture absorption.

The internal trims were applied over the frames, and the absorbents in layers in one or more of the positions, (a) directly against the skin between the stringers, (b) over the stringers and (c) over the frames directly beneath the internal trim.

A selection of the treatments tested is listed in Table 4, and the results of the sound insulation measurements are shown in Fig. 9 in terms of the increase in the sound reduction factor over that of the untreated panel, averaged for four frequency ranges 100 to 200, 300 to 700, 1,000 to 2,000 and 3,000 to 4,000 c.p.s.

From Fig. 9 it will be observed that there is a general tendency for the improvement of insulation to increase with the weight of the treatment, as indicated by the curves drawn for the four frequency ranges.* There is, however, considerable irregular divergence from the lines as a result of the other variables concerned—the types of treatment and the nature of the materials.

In the selection given no treatments employing the leather internal cover are included. Both the leather cover and the Vynide cover were non-porous and the results for the former are similar to those for the latter, being on the average one or two decibels higher owing to the rather greater weight of the leather. The cloth cover, on

* These are not intended to be mean curves, and in drawing them some of the poorer treatments have been deliberately ignored.

TABLE IV

DESCRIPTION OF SOUND-PROOFING TREATMENTS

Ref. No.	Treatment			Interval Cover	Total additional Weight (lb./sq. ft.)
	Between Stringers	Over Stringers	Over Frames		
2a	—	Fiberglas (V.M.)	Fiberglas (M.V.)	Vynide	0.185
4a	—	Fiberglas (V.M.) + do. (plain)	Fiberglas (plain) + do. (M.V.)	Vynide	0.24
4c	—	do.	do.	Cloth	0.29
6c	Woolfelt	Fiberglas (V.C.)	Fiberglas (C)	Cloth	0.395
7c	do.	do.	Vynide + Fiberglas (C)	Cloth	0.49
8a	—	Kapok (V)	—	Vynide	0.17
10a	—	Kapok (V)	Kapok (V)	Vynide	0.295
10c	—	do.	do.	Cloth	0.245
11c	—	Kapok (V) + Kapok	Kapok + Kapok (V)	Cloth	0.415
12	—	Kapok (V) + Vynide	—	—	0.17
12c	—	do.	—	Cloth	0.32
16a	—	Stonefelt	Stonefelt (CC)	Vynide	0.535
16c	—	do.	do.	Cloth	0.585

V—V film stuck to one side. When the material was applied over the stringers the V film was towards the skin. When the materia was applied over the frames the V film was towards the internal cover.

V.M.—V film stuck to one side and muslin to other side. V film towards skin.

M.V.—V film stuck to one side and muslin to other side. V film towards internal cover.

C—Calico stuck to side towards internal cover.

V.C.—V film stuck to one side and calico to other side. V film towards skin.

CC—Calico stuck to each side.

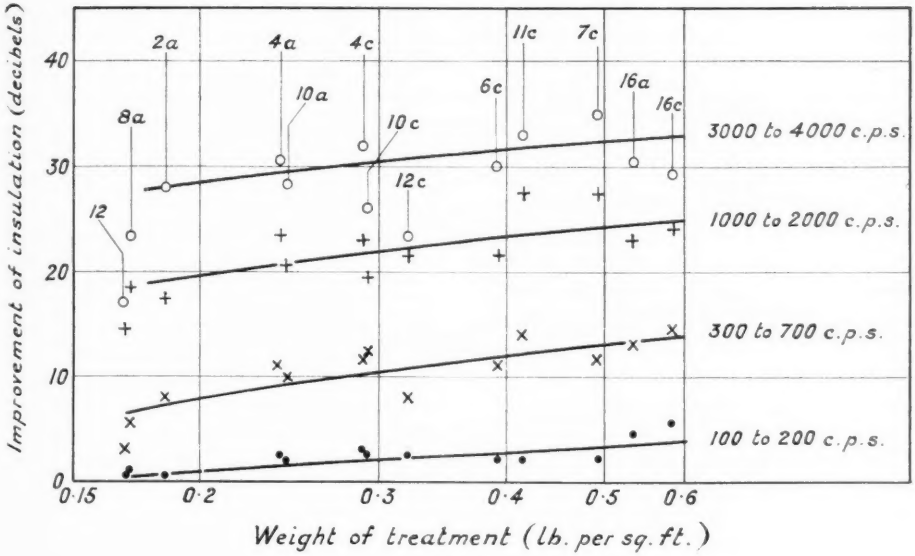

Fig. 9

Improvement of insulation afforded by a number of practical sound-proofing treatments to an aircraft panel with skin of 0.5 lb. per sq. ft.

the other hand, is porous, and in spite of its greater weight gives rather lower insulation at the high frequencies than the Vynide cover in treatments involving two layers of absorbent. Compare, for instance, 10a and 10c (two layers of Kapok) and 16a and 16c (two layers of Stonefelt). With four layers of absorbent, however, the porosity of the cover has less effect. With four layers of Fiberglas, for instance (4a and 4c), the cloth cover is 1.5 db. better than the Vynide cover in the frequency range 3,000 to 4,000 c.p.s. and only 0.5 db. worse in the range 1,000 to 2,000 c.p.s. Treatment 11c (four layers of Kapok with the cloth cover) also shows comparatively high values in the two high frequency ranges. The corresponding treatment with the Vynide cover was not tested.

Comparison of the three different types of absorbent material, Fiberglas, Kapok and Stonefelt, in similar treatments, shows that, for equal weights, Fiberglas is the best and Stonefelt the worst. Thus, referring to Fig. 9, 2a (two layers of Fiberglas) is better than 8a (one layer of Kapok) of approximately equal weight; 4a and 4c (four layers of Fiberglas) are respectively better than 10a and 10c (two layers of Kapok); and 11c (four layers of Kapok) is better than the considerably heavier treatment 16c (two layers of Stonefelt).

Most of the treatments tested were of the same general type, one or more layers of absorbent over the stringers and one or more layers over the frames, finished by the internal cover. This distribution of the absorbent material between the two positions, and the utilisation of the full depth of about 4in. between the front of the frames and the skin, appeared to be the most satisfactory. Thus a treatment (12) consisting of a single layer of Kapok with Vynide stuck to it applied over the stringers was much inferior to the treatment 8a employing the same materials, but with the Vynide cover over the frames. With a cloth cover over the frames added to 12, giving 12c, the insulation was again relatively poor at the high frequencies.

The effect of a non-porous membrane

applied directly over the frames below the inner layer of absorbent, in a treatment employing the porous cloth cover, was tested in one case. For this purpose the Vynide was used, and introduced into treatment 6c giving 7c. It will be seen from Fig. 9 that the resulting increase in the insulation in the two higher frequency ranges was of the order of 5 db., and considerably greater than is to be attributed to the increase of weight alone. This treatment showed an improvement of insulation over that of the untreated panel of 35 decibels in the frequency range 3,000 to 4,000. In this range the sound reduction factor of the treated panel was 60 db., for a total weight of treatment and outer skin of about 1 lb. per sq. ft. This is as good as that provided, in the same frequency range, by a 4½in. brick wall of weight about 50 lb. per sq. ft.

3. *Sound Absorption*

Measurements of the sound absorption coefficients of these treatments showed that the absorption depended much more upon the nature of the internal lining than on the type of sound-absorbent material employed in the treatment. There was very little difference between treatments having the non-porous Vynide or leather covers, but considerable difference between these and treatments having the porous cloth cover. Table V shows the range of values measured for the absorption coefficients of treatments incorporating either Fiberglas or Kapok, and consisting of one or more layers of absorbent over the stringers and one or more layers over the frames and the internal cover. The table also includes the values, calculated from equation 5, of the contribution of the absorption to the total noise reduction which the treatments would afford in the furnished cabin already considered.

The low absorption at high frequencies of treatments with a non-porous cover leads to no increase in the total absorption in the cabin when the treatment is applied, and consequently to no noise reduction from this cause, as against a reduction of about 3 db. from a treatment with a porous cover. Thus the rather smaller sound reduction factor of a treatment with porous cover is more than offset by the effect of its greater absorption coefficient.

If the full advantage of the high values of insulation of sound-proofed walls—of the order of 60 db.—is to be secured, it is vitally important to ensure that sound entering by other paths is less than that transmitted through the sound-proofed area. There should be no difficulty in sufficiently reducing noise transmission through the floor by a suitable sound-proofing treatment to the skin below, and the remaining paths which require consideration are the windows, ventilation and heating ducts, and gaps at imperfectly fitting doors or opening windows.

4. *Windows*

In an untreated cabin the sound entering

TABLE V

ABSORPTION COEFFICIENTS, AND NOISE REDUCTION DUE TO ABSORPTION, FOR SOUND PROOFING TREATMENTS IN A FURNISHED CABIN

Frequency Range	Cloth Cover		Vynide or Leather Cover	
	Absorption Coefficient	Noise Reduction (db.)	Absorption Coefficient	Noise Reduction (db.)
100-200 c.p.s.	0.7 to 0.8	4 to 4.5	0.55 to 0.7	3.5 to 4
300-700 c.p.s.	0.7 to 0.8	3.5 to 4	0.5 to 0.65	3 to 3.5
1,000-2,000 c.p.s.	0.5 to 0.6	3.5 to 4.5	0.15 to 0.2	1.5
3,000-4,000 c.p.s.	0.5 to 0.6	3 to 3.5	0.05	0

TABLE VI

CONTRIBUTION TO TOTAL NOISE LEVEL OF SOUND TRANSMITTED THROUGH WINDOWS (WINDOW AREA 0.08 OF WALL AREA)

Component	Sound Reduction Factor in Frequency Range			
	100-200 c.p.s. db.	300-700 c.p.s. db.	1000-2000 c.p.s. db.	3000-4000 c.p.s. db.
1. Untreated walls (skin 0.5 lb./sq. ft.)	9	13	22	25
2. Sound-proofed walls (additional 0.5 lb./sq. ft.) ..	12	26	46	57
3. Single window (2 lb./sq. ft.)	17	22	31	37
4. Double windows (total weight 2 lb./sq. ft.) $\frac{1}{4}$in. spacing	17	18	41	48
5. Ditto 1in. spacing	15	31	47	51
6. Ditto, ditto + absorbent lining to reveal	15	31	49	57
Combination	**Increase in Noise Level Due to Transmission by Windows (Decibels)**			
Untreated walls (1) and single windows (3)	0	0	0	0
Sound-proofed walls (2) and single windows (3) ..	0	1	5.5	9.5
Ditto(2) + double windows (4)	0	2	1	2
Ditto + double windows (5)	0	0	0.5	1
Ditto + double windows (6)	0	0	0	0.5

via the windows, which are normally of greater superficial weight than that of the skin, is negligible compared with that entering through the rest of the walls. With a sound-proofing treatment on the walls the sound transmitted by the windows in spite of their comparatively small area, may exceed that coming through the rest of the walls. In order that the contribution from the windows should not raise the average noise level by more than 0.5 db., the sound reduction factor of the windows should not be more than 4 db. below that of the walls if the window area is 5 per cent. of the total wall and ceiling area, or not more than 1 db. below that of the walls if the window area is 10 per cent. of the total area. As an illustration of the effect, Table VI gives typical values for the sound reduction factors of some walls and windows, together with the calculated values, for a cabin in which the window area is 8 per cent. of the wall and ceiling area, of the increase in the noise level due to the sound transmitted through the windows, *i.e.*, $10 \log_{10} (t_1 + .08t_2)/t_1$ where t_1 and t_2 are the transmission coefficients of the walls and windows respectively.

Thus, with sound-proofed walls and single windows of weight 2 lb. per sq. ft. (*e.g.*, $\frac{3}{8}$in. Perspex) the effect of the sound-proofing treatment is materially reduced at the high frequencies. Some measurements in an aircraft in flight, with windows of this type, showed that close to a window the noise level was higher than that near to an adjacent sound-proofed wall panel by amounts increasing from about 1 db. in the octave band from 300 to 600 c.p.s. to 12 db. in the octave band 4,800 to 9,600 c.p.s. In the lower octave bands the noise level near to the window was one or two decibels less than that near the wall, as would be expected from the figures of Table VI. With double windows, however, insulations sufficiently high can be obtained for the full advantage of the sound-proofing treatment to be realised.

5. *Ventilation Ducts and Door and Window Seals*

If the sound reduction factor of the cabin walls is 60 db. at high frequencies, the

fraction of the incident sound energy which is transmitted into the cabin is $1/10^6$. Thus a small opening, of transmission coefficient unity, would transmit as much sound as the whole of the walls if its area was $1/10^6$ of that of the walls, *i.e.*, a small fraction of a square inch in a cabin of normal dimensions. Fortunately the transmission coefficient of such a small opening is less than unity, but the ease with which transmission through gaps at imperfectly fitting doors or windows, or through ventilation ducts communicating with the outside, may completely nullify the effect of a highly efficient sound insulating treatment will be realised.

In a non-pressurised cabin, in which the fresh air supply is from ducts with forward facing openings towards the nose of the machine, the high frequency noise entering through the fresh air inlets may well considerably exceed that coming through the sound-proofed walls. In one case examined, where the inlet louvres could only be imperfectly closed, the high frequency noise in the cabin was reduced by 3 db. on closing the inlets. Thus the high frequency noise entering via the duct was at least equal to that entering by all other paths. The remedy is to line a section of the duct, between the external opening and the first opening to the cabin, with sound absorbent material such as Fiberglas. In the absence of specific test data, precise figures for the design of such sound attenuating ducts cannot be given. The attenuation per foot run, however, increases with the ratio of the perimeter to the cross-sectional area, so that a narrow rectangular duct is better than one of square or circular section of the same area. With an internal width of 2in. an attenuation of the order of 10 db. per ft. would probably be obtained at the high frequencies, so that treatment of a few feet only would be required.

The location of the external opening should be chosen so as to be shielded as far as possible from the main sources of noise—engine exhaust and airscrew. With an extract duct, opening towards the tail of the machine, where the external noise is less, silencing in the duct may still be necessary, though to a smaller extent. In pressurised cabins similar absorbent lined sections of duct may be required between the compressor and the cabin, and on each side of the fan of the recirculating system.

It is advisable to confine the air to a duct, rather than to utilise the space between the inner and outer layers of sound-proofing for the circulation of air. With the latter expedient the inner layer of sound-proofing must necessarily be pierced at some points to allow the air to pass into or out from the cabin, with a consequent reduction in the insulation against outside noise at these points.

CONCLUSION

With an aircraft cabin having a skin of weight about 0.5 lb. per sq. ft., an efficient sound-proofing treatment weighing a further 0.5 lb. per sq. ft., double windows, effectively silenced ventilation, and adequate seals at door and window openings, a noise reduction between the outside and inside ranging from 10 db. at low frequencies up to more than 60 db. at high frequencies is possible. Little further improvement on these figures is likely to be obtained, even with considerably greater weights of treatment. Direct measurement of the noise outside an aircraft in flight is not practicable, but judging from measurements on the ground and in aircraft cabins in flight, it appears that, with engines with ejector-stub exhausts, a sound-proofing treatment alone will not be sufficient to reduce the noise to below the maximum recommended levels for civil passenger machines. Reduction of noise at the source is also necessary, and in addition every endeavour should be made in design to locate the passengers in the less noisy parts of the aircraft—away from the region near to, and a little behind the plane of the airscrews.

In an aircraft with cruising speed of about 200 m.p.h., operating at an engine speed of about 2,000 r.p.m., and horse power per engine about 700, and with three- or four-bladed airscrews at tip speeds in the region of 600ft. per sec., the predominant source of noise is the engine exhaust. With open stub exhausts this is likely to be greatest in all frequency bands except the lowest, which is below the explosion frequency from the stubs on one side of the engine, but includes the fundamental frequency of the airscrew noise. The exhaust noise can be considerably reduced by the use of exhaust manifolds, as is shown in Fig. 5, and these may be necessary in spite of any reduction of engine efficiency and loss of exhaust thrust which they may entail. When manifolds are used, the opportunity should be taken of locating the exhaust discharge at a point screened from the passenger cabins by the wing or engine nacelle.

With exhaust manifolds and an efficient sound-proofing treatment there is a reasonable prospect of the recommended standard being achieved at normal cruising speeds. The exhaust noise may still be the most important in the frequency range from 75 to 600 c.p.s., but at the higher frequencies aerodynamic noise may predominate. This source of noise is likely to become of increasing importance as cruising speeds are increased, and will require more attention in the future than has hitherto been given to it.

ACKNOWLEDGMENTS

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DISCUSSION

The CHAIRMAN: He complimented the author on the paper and congratulated him on his clear exposition of a difficult subject. Most designers had experienced noise problems, particularly during the past few years and on their behalf he expressed his gratitude for the ready way in which Mr. Fleming had always given them help. On behalf of the Society he expressed deep appreciation of his work on sound and his very fine paper.

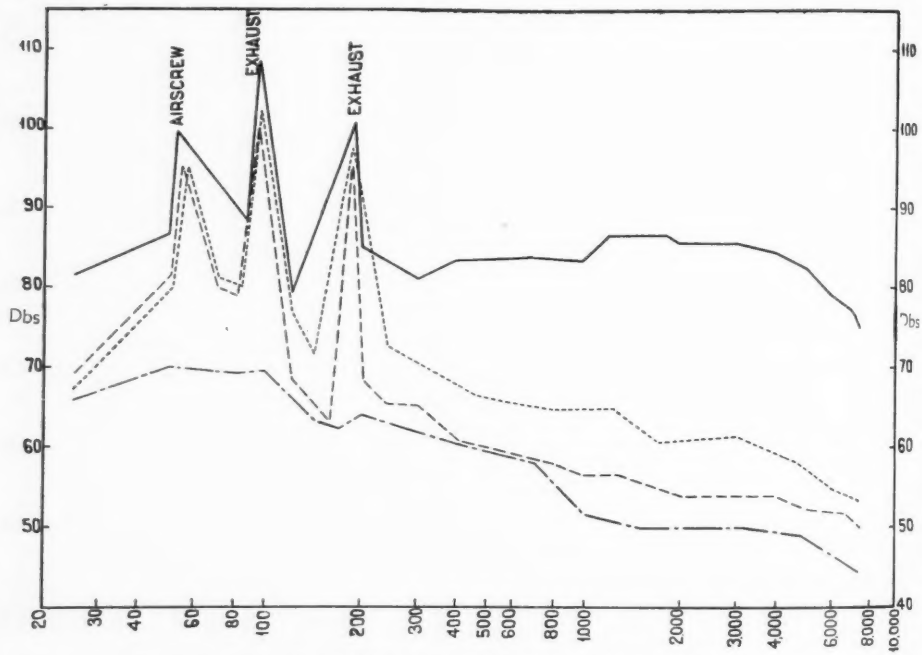
Would Mr. Fleming speculate a little for the benefit of designers and all others concerned with high-speed flight? They had been told that the high-frequency noises would constitute a problem. What was the intensity of the problem when the awkward stage was reached at which M=1? Would the crew of the aircraft travelling at that speed be deafened by noise, or would they be travelling so fast that they would hear nothing?

DR. E. W. STILL (Rolls-Royce Ltd., Assoc. Fellow): He thanked Mr. Fleming for all the work he had done for Rolls-Royce. The turnover from war to peace conditions had left them with the task of transferring the exhaust energy from speed to silence, and it was a difficult problem. The Spitfire for instance, was gaining 30 m.p.h. in speed as the result of its ejector exhaust; they did not want to lose that increase of speed. Perhaps they had gone too far in showing what could be done with the exhaust in the way of speed. At the same time, they must try to retain the high speed and with it silence, which was very difficult.

They were somewhat at a loss to understand Mr. Fleming's slides based on the octave analysis. As engine people they were blamed, of course, for all the noises in civil aircraft, whether those noises were due to the airscrew, the engine or the aeroplane structure itself; they wanted to know where the noise came from, and how far they could help in its reduction. They had tried first an objective noise meter on a Spitfire Griffon installation, and also on a Firefly with a beautiful flame-damped exhaust. The Spitfire was a most noisy aircraft, yet the noise meter had indicated that it was quiet; the Firefly, which was in fact very quiet indeed, was shown by the noise meter to be the noisiest.

From that experience he had concluded that there was more in it than just the

GRAPH I.

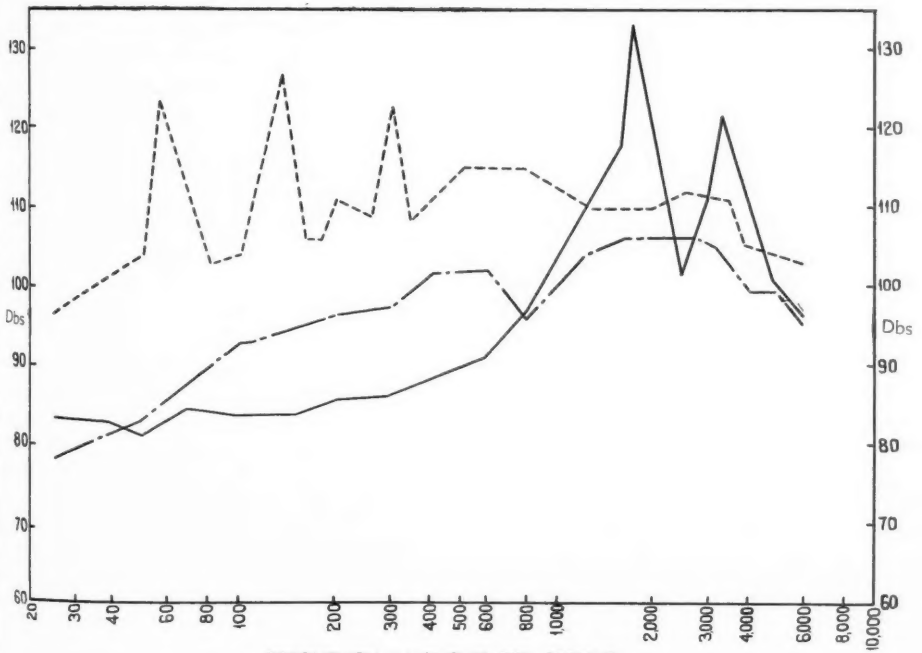


measurement of the average noise, which was the noise measured by the noise meter. He believed most noise meters read the root mean square value. In the case of ejector stubs, they should be able to measure the impact of the noise coming out of the stub, because people were worried about that quite as much as they were about the average value of the noise. What was Mr. Fleming's opinion on that matter? So far as he knew there was no noise meter in England which gave peak values, if these were the values the ear heard.

The octave noise meters had been of no value to him because they would not indicate whether the noise that was measured came from the airscrew or the ejector exhaust, or whether it was aerodynamic noise. But largely as the result of Mr. Fleming's help, they were able to borrow an instrument which gave a much closer analysis. Some of the answers obtained when using that instrument were shown in the accompanying graphs, where the frequency was plotted against decibels in the same way as in Mr. Fleming's graphs.

That gave a very different picture from the nice smooth lines shown in Mr. Fleming's diagrams. It did indicate to engine designers and the airscrew people the parts of the problem which they had to tackle; it gave a rough idea of the values of the various components with which they were concerned. All the lines related to an aircraft in which the same engine was used, and it was plain that there was quite a lot of noise aerodynamically. He would like to know whether, as cruising speed was increased—as

GRAPH II.



GENERAL RADIO SOUND LEVEL METER TYPE 759B AND ANALYSER 760A
SOUND READINGS TAKEN ON GROUND

- - - - - Piston Engine and Airscrew. ———— Jet A. —— - - —— Jet B.

it would be with the development of the jet engines—there would not be quite serious trouble, in spite of the means of sound-proofing of which they had become aware.

The American opinion was that they would find it difficult indeed to silence the exhaust of large engines of 3,500 h.p. and thereabouts. He could not appreciate why the silencing of those engines should be more difficult than the silencing of small engines; the problem was exactly the same in both cases.

The engine builders could eliminate a number of the peaks in the noise curves, but there would remain some high noise values on the airscrew side, and he would like to hear opinions from the airscrew manufacturers as to what they proposed could be done on the problem. The engine builders could deal with the nasty peaks due to the exhaust system, given time, although it might cost a little in speed; those noise peaks were important in a civil machine, for they were the most fatiguing. If the level of those noises could be reduced, the passengers might no longer complain. So far, he was referring only to piston engines.

Comparing the noise values between the piston engine and two types of jet engines, his curves for the jet engines were not based on flight figures, for they had not been able to measure the jet noises in flight yet. With the piston engine most of the noise was at the low frequency end; with the jet engine the position was exactly the reverse. Inasmuch as the figures related to a ground run, the curves perhaps looked rather poor compared with the previous curves of flight results, and it should be compared with a curve representing the results for a ground run for the piston engine, as shown in Fig. 2. For the jet engine there was virtually a rising line with increase of frequency and an occasional peak, which was usually a function of the impeller blade x the revs.

The curves indicated the troubles which had to be faced in connection with the jet engines, and the reduction of high-frequency noise would not be simple. In the plain jet engine the noise levels were very high at the high frequencies, and although it responded to sound-proofing at the high-frequency end, the solution of the problem would not be easy.

WING COMMANDER T. R. CAVE-BROWNE-CAVE (Fellow): He thought that it would be difficult to reduce the noise of aircraft to a moderate level but there was much greater chance of being able to eliminate those characteristics of the noise which made it offensive and tiring. The problem of noise was essentially one which depended upon the effects produced on human beings. The physical measurement of the magnitude of the noise was not necessarily the factor of greatest importance.

Shortly before the war he had made some tests for the Air Ministry to decrease the offensiveness of the noise caused by training aircraft passing low over houses in the neighbourhood. In these tests he had taken as a criterion the effect produced upon a group of individuals who were invited to assess the offensiveness of the various types of aircraft as they flew over at a given height and under given conditions. It had appeared to him that the offensiveness was mainly due, not to the magnitude of the noise, but to the high pitch component of sound of comparatively short wavelength. This sound was due not to the airscrew but to the exhaust, which in most cases was delivered vertically downwards. The high-frequency noise had a wavelength of about one foot and could, therefore, be screened from the ground if the exhaust were discharged above rather than below the wing.

Six types of training aircraft had their exhaust systems modified in this way and were then judged by a panel of judges collected from the neighbours who lived near the aerodrome and had been troubled by the noise. The panel was posted on the aerodrome and two aircraft of each type were

flown over in succession, one aircraft of each pair having the standard exhaust system, the other having the exhaust outlet above the wing. The aircraft passed over in three different ways. First, immediately after taking off, then under cruising conditions and finally diving at full speed over the panel of judges.

The panel was invited to classify each aircraft into one of four groups: "very bad," "offensive," "harmless" and "very good." They did not know the identity of each aircraft which passed over or whether it had the standard or the modified form of exhaust.

The conclusions reached were very interesting from two points of view. The markings given by various individuals were surprisingly uniform, although there was no discussion between individuals and in each case every judge marked the modified aircraft better than the identical aircraft having the standard exhaust system.

The result was considered so satisfactory that the BBC arranged to record the noise of one pair of aircraft and invited him to give a short talk describing what had been done. The talk was illustrated by reproductions of the noise of the two aircraft but met with varying success. The record when played over in the studio showed a marked difference in offensiveness between the standard and the modified exhaust system. On the other hand, certain of those who heard the broadcast could make little distinction between the two. Subsequent enquiry showed that many listeners who disliked high-frequencies kept their sets adjusted so as to cut out high-frequency sounds. They had, in fact, done on their sets exactly what had proved effective on the aircraft.

He suggested that there were three outstanding points on the scale of offensiveness. At about 120 db. the magnitude of the noise was so great that it caused physical pain. This noise level was comparatively seldom experienced. The next point was that at which the noise was sufficiently loud to interfere with conversation. It then caused fatigue and became a source of annoyance on that account. This was between 60 and 70 db. The third point did not depend greatly upon magnitude but principally upon the nature of the noise, its pitch and particularly its pattern and variation. A noise which forcibly arrested the attention might be more fatiguing and annoying than one of much greater magnitude.

Therefore he advocated a systematic investigation to determine the characteristics of a noise which made it offensive and fatiguing. It was now possible to record noises accurately so that they could be reproduced with complete fidelity. A number of composite noises should be recorded and should then be assessed by a wide range of individuals in order to determine their comparative offensiveness. An analysis made with the same thoroughness as the Social Survey to which he had referred ought to yield valuable conclusions as to the characteristics which constituted offensiveness. These were the factors which really mattered and in many cases there was a better chance of removing the offensive components than of reducing the magnitude to a harmless level.

He referred to the results given by the author from observations made in an engine test house. During the machinery trials of the Airship R.101 it was found that the noise of a single engine running while the airship was in the shed reached a very serious level in the passenger accommodation. When the ship was transferred to the mooring mast so that there was reverberation from the ground only, the noise level was harmless. When the airship was in flight at some distance from the ground, the noise level in the passenger accommodation was so low that it was possible to talk to a person sitting 10 or 12 feet away. The importance of reverberation was, therefore, very great.

He emphasised the importance of an investigation to determine the characteristics of a noise spectrum which contributed to the

offensiveness as judged by its effect on human beings.

Mr. A. W. Morley (Assoc. Fellow): In the paper the problem was approached critically. Mr. Fleming had the background of the N.P.L. rather than the background of the aeronautical engineer, and in conversation he had left the impression that there was not much more he could do towards silencing aircraft, provided that all the measures for sound proofing of which they were at present aware were taken. As Mr. Fleming had pointed out, it was difficult to do anything on the engine side unless the propeller people also managed to do something. Thus it seemed that they were almost at a deadlock in respect of the silencing of aircraft, and they had rather to adjust their ideas as to what silencing would really be worth.

The prime duty of aircraft was to enable people to travel from place to place swiftly, not necessarily comfortably. There was a comfort requirement, but the degree of comfort that was provided must relate to endurance; *i.e.*, there would be a minimum standard of silencing which would depend on endurance, not on the range of the aircraft. Perhaps that minimum would have to increase as the endurance of the aircraft increased.

The sort of information which was lacking, but which was required in order to complete the picture, was that such as Wing Commander Cave-Browne-Cave had mentioned, relating to the actual physiological effects produced on human beings, how much the passengers were willing to pay for silencing; and that information was very difficult to obtain. At the beginning of the war they had flown well-trained R.A.F. crews for several hours in noisy aircraft and, according to the physiological tests, there was no real danger in so doing.

The recommended maxima of noise levels in Figure 2 of the paper seemed to be based on a shrewd idea of what might be done in piston-engined aircraft. Certainly the curve would not apply to the faster turbine-propelled aeroplanes of the future, and that fact ought to be made clear.

On Fig. 5, which gave the results of noise measurements on the Rolls-Royce engines, there was an overall difference of noise of some 15 db.; but the ordinary reader of the paper had no idea how the differences shown were brought about. If it were possible, say, to point out basic principles the application of which would assist in silencing an engine, it would add greatly to the value of the figures.

In the past he had found that the character of the propeller noise was very different according to whether the aircraft was in flight or on the ground; it was fairly obvious why that should be so, in the light of their present ideas of noise. It was also found that the rate of increase of noise with increase of propeller tip-speed was much lower when in flight than when on the ground. For example, on the spinning plant the overall noise measurements had increased at the rate of 10 phons per 100 ft. per second, whereas in flight the increase was of the order of 6 phons per 100 ft. per second increase of tip speed, which had helped very much in connection with the airscrew noise problem. He could never match up the noise measurements made in flight and on the test bed, largely because of the reflections, which played so important a part in the matter.

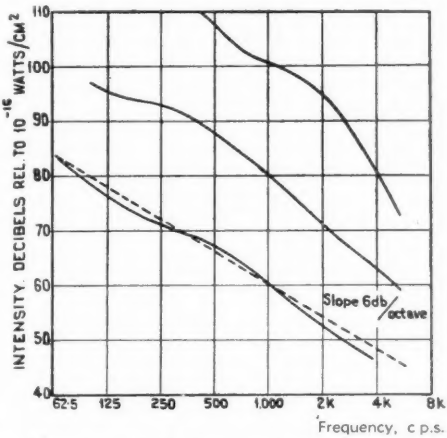
Gutin's paper on airscrew noise, which Mr. Fleming had mentioned, gave a valuable analysis, and fitted in well with the ground level measurements of propeller noise. It would be interesting if a mathematician could apply the same type of analysis to a propeller in flight, in order to see whether or not the definite difference in character was likely to be helpful.

As to aerodynamic noises, Mr. Fleming and other observers agreed that, at speeds in the

region of 200 m.p.h., those noises were not much below the propeller noise or the engine noise. It seemed rather unlikely that propeller noise and engine noise would increase much more with increase of aircraft speed; but aerodynamic noise was almost bound to increase. It depended on a compressibility effect, and the important pressure term was proportional to the fourth power of the velocity; so that the sound intensity, being proportional to the square of the pressure, would increase as the eighth power of the velocity. This meant that when aircraft reached cruising speeds, of say, 400 m.p.h., the aerodynamic noise would be by far the most important noise. He did not feel that such a conclusion would prove to be true, by reason of the improvements which would be effected in aircraft which would make it possible to fly at the higher speeds; the aircraft would be cleaner and, therefore, less prone to turbulence noises. It would be valuable to have an analysis of aerodynamic noise, because it would influence the amount of work that had to be put into the silencing of the engine and propeller for future aircraft which would travel at the highest speeds.

Mr. J. R. LEACH (Royal Aircraft Establishment, Assoc. Fellow): Mr. Morley had suggested that Fig. 2 must be modified to fit the conditions which would arise with the development of the jet engine, but the curves showed recommended maximum levels. If they were comfortable levels, the introduction of the jet engine should not mean that they must change their requirements regarding silencing. To change the recommended levels to suit the type of engine used seemed to be taking quite the wrong step.

Mr. W. MAKINSON (Royal Aircraft Establishment): He agreed with Wing Commander Cave-Browne-Cave that a lot of good would accrue from an investigation into the effects on personnel of the different parts of the spectrum of aircraft noise; but such work was difficult, for it was not easy to get even a comparative measure of things in those purely subjective studies. Such work had been attempted, however, in the U.S.A. by Donald A. Laird, a psychologist; he had subjected observers to different intensities of pure tones at different frequencies and had measured the relative annoyance of those sounds. He had obtained curves such as the following, showing that annoyance tended to increase with increase of frequency.



Curves of equal annoyance for pure tones

During the early part of the war he had had occasion at Farnborough to review various published works on the subject and had come across a paper written by Dr. Postlethwaite, of the R.A.E., dealing with the investigation of the effects, not of noise, but of vibration, on personnel. Aircraft vibration and noise were, of course, very much allied in their origin and nature, the only real differences being those of frequency and amplitude. Dr. Postlethwaite had correlated the results of many observers and had postulated a law for the relative effects of vibrations whereby the sensation produced was said to be proportional to the logarithm of the acceleration to which the person was subjected. In other words, for a given amplitude of vibration the annoyance increased with the increase of frequency. Plotting out the results for different ampli-

tudes and frequencies, he had obtained a series of curves for the results of the different observers, which showed good correlation between the logarithm of the acceleration and the sensation produced.

Thinking that there might be some correlation between the effects of vibration and noise, he had done the same thing with Laird's equal annoyance curves and had found that they could be regarded as a continuation of the equal sensation curves of Postlethwaite extrapolated to the higher frequencies. There appeared, therefore, to be a similar correlation between the acceleration of the air particles in the sound wave and the annoyance value. He did not know whether there was a psychological reason for this, but the agreement seemed too striking to be a mere coincidence.

With regard to the effects on speech, he agreed entirely with Wing Commander Cave-Browne-Cave that the comfort of a long trip in an airliner would be almost entirely defined by the difficulty or otherwise of carrying on conversation. When crossing the Atlantic by air, for instance, if passengers wanted to play bridge they would not want to have to shout " Three spades " at the top of their voice but to make their call in a perfectly normal manner. If aircraft noise could be rendered sufficiently tolerable to enable passengers to converse normally, he believed that that would go 90 per cent of the way towards the solution of the problem. It had been found in the laboratory that that could be done by attenuating the noise in the middle and upper regions of the spectrum leaving the lower frequency sounds practically unaffected. There would no doubt still be some people who were allergic to low-frequency sound. Again, the work of S. S. Stevens at Harvard had led to the suggestion that the direct effects of noise on psychomotor efficiency were very small; this aspect of it could be more or less ignored provided that it did not interfere with any operation one wished to perform. If, however, the noise were such that more effort had to be made and, for example, they had to speak loudly, it would be fatiguing.

A most important matter in aircraft powered by twin or multi-reciprocating engines was to eliminate beats. If there were beats, particularly between twin engines, periods of really intense sound were experienced; although the average noise level might measure 120 db., beats could take it up to 130 db. or peaks. Further, the intense vibration could in turn cause all the fittings on the aircraft to become loose, and they would become secondary sources of noise. If any one of those fittings, such as an ash tray or something of that sort, were close to the ear, the noise would become very annoying.

Wing Commander CAVE-BROWNE-CAVE: Would it be right to assume from Mr. Makinson's curve that the high-frequency noise was less annoying than the low-frequency noise?

Mr. MAKINSON: The high-frequency noise was definitely said to be more annoying than the low-frequency noise. His lines were lines of equal annoyance; in other words, at the higher frequencies a much lower intensity of noise would give the same degree of annoyance.

Mr. J. A. C. WILLIAMS (Assoc. Fellow): Mr. Fleming had said that the largest contribution of noise from airscrews came from the fundamental; was there a large contribution from sub-harmonic sources?

He felt that some people were fearing too much aerodynamic noise from fast-flying aircraft; he suggested that such aircraft would have laminar flow sections and, therefore, the anticipated large increase of noise from the aerodynamic source might not materialise. Doubtless there would be an increase of noise, but it would not be so great as was feared.

At the moment those concerned with wind tunnels feared that the noise in the wind

tunnels would affect the transition point on the wing when laminar flow sections were tested.

Mr. G. H. LEE (Handley Page Ltd., Assoc. Fellow): Could Mr. Fleming explain the very strong directional or focussing effect which was always so puzzling in connection with aircraft noise? He was under the impression that the sound spread out more or less uniformly in most directions if the source of the sound were small as compared with the wavelength.

At a demonstration of the Vampire some time ago he had noted that when it came towards the observer it did not appear to make much noise, whereas when overhead and retreating the noise was terrific. If there were a strongly focal effect of sound, there was some hope of being able to shield the people travelling in the aeroplane, even if it upset the local residents.

Most aerodynamic noises must be due to factors such as small beaded parts around the windows and badly fitting components generally, rather than to the main flow of air over the wings. Had Mr. Fleming any idea how much noise ought to be made by air flowing over a flat surface? If that were known and could be compared with the amount of noise an aeroplane did in fact produce, they could decide how much of the noise was due to window fittings and the like.

Mr. E. J. RICHARDS (Vickers-Armstrongs Ltd., Assoc. Fellow) *contributed*: As the result of his experience with the Vickers Viking, it had become apparent that the amount of reduction that could be done on a prototype was strictly limited and that this would be especially true on future aircraft in which exhaust noise was not the predominating factor. A further point was that they really could not afford to add much more sound-proofing material and that all future improvements must be by reduction of the source of the noise, and by better distribution of the lagging.

It could, therefore, be taken as a basic truth that any reduction of noise must be carried out " in the project stage " and considerations of cabin layout, airscrew tip-speed, tip clearance, engine suitability, and surface finish and leaks must all be considered from the noise point of view at this stage. To do so, very much more quantitative methods of noise estimation should be available and future researches should have this aim in view.

Consider the effect of cabin layout. To obtain the quietest cabin it was obviously necessary (for a given weight of structure and acoustical material) to put as much weight as possible near the source of sound, and it was apparent, for instance, that they could reduce the noise quite considerably if they could put the luggage compartment in the disc of the airscrews, if they extended the bulkheads to the outer skin and if they sealed the doors from the cabin to the luggage bay. How much could be gained by this and by graded acoustical treatment along the walls depended entirely on how well they could estimate the noise spectrum at all points along the shell of the aircraft; this would certainly require far better methods of estimating airscrew noise than existed at the moment. These calculations or empirical relationships would need to include the effects of airscrew tip-speed, plan form and tip clearance as well as jet directional effects.

Secondly, they had the suitability of the engine. They had heard many stories of turbine engines being either very quiet or very noisy and it was reassuring to obtain concrete figures given in the lecture. But they could not decide on the complete suitability of this type of engine or on the gear ratio to be used until they had obtained a fair number of basic tests at all points around the jet engine. It was clearly useless to sacrifice the poor take-off qualities arising from a reduction of cruising tip-speed, if, in fact, the predominating noise was that of the engine impellor. And it must be remem-

bered that on a jet engine they could not cut a little off the propeller blades in the same way as was the practice with airscrews.

Lastly, there were the effects of speed, surface finish and leaks on the high-frequency aerodynamic noise. Tests on the Viking with power on and off and with the propellers feathered had shown (as mentioned on page 653 of the Lecture) that above 600 cycles per second, all the noise came from the flow of air over the aeroplane (and possibly vibration). He must admit that until he saw Fig. 2 of the lecture he attributed this whistling noise (which was about 5 decibels higher than the recommendation of Fig. 2) very largely to leaks through the access doors and through the cold air entries. However, if, as he suspected, the 4-engined aircraft of Fig. 2 was a pressurised type without leaks, this conclusion would appear to be wrong and over the higher frequencies they had a real problem to face, particularly at high speeds. To be fair, he must say that this sound level was not really objectionable on the Viking and was a vast improvement on old types, but if the noise intensity was increased as the square of the speed, the 6 decibels increase on this at 400 m.p.h. might well be very uncomfortable on a long journey. The aircraft designer therefore needed quantitative data which would allow him to estimate the aerodynamic noise on high-speed aircraft and research on this subject should aim not only at producing this data, but also at explaining the major sources of the aerodynamic noise.

These were their needs; he would be very pleased to hear from Mr. Fleming whether the researchers of the future were going to satisfy them. He had shown in his excellent paper the way to go about it, but unfortunately, the resources available to do so in this country, were regrettably small.

Group Captain A. F. Scroggs (Fellow) *contributed*: He would like to emphasise the importance of the physiological aspect of this problem, for it was the effects of noise, rather than the noise itself, that they had to try to eliminate, and scientific investigation might be misleading unless they knew what these effects were, and just what kind of noise it was that produced them. It was a pity that no one had spoken from the medical point of view.

Most people seemed to treat the matter solely on the score of the "nuisance value" of aircraft noise as it affected passengers and those on the ground. It seemed to be generally agreed that the higher frequencies were the most objectionable, but he suggested that this was not necessarily true of other less obvious but perhaps even more important effects, particularly in those who, like flying crews, were exposed to them day-in, day-out, for long periods. He had reason to believe that regular flying did accelerate the normal dulling of hearing associated with advancing age which, with the increasing use of radio, might be a vital matter for a pilot and even render him prematurely medically unfit for full flying duties. He did not think it was necessarily the high-pitched sounds that did this, and he could think of two types of aircraft which made him appreciably deaf after a couple of hours or so; and in these, low-pitched noise appeared to predominate. The effect of "impact noise," as he could confirm from personal experience, might be particularly severe, and as Dr. Still pointed out it was not fairly assessed by any instrument which measured the average noise level. By all means let them quieten the passengers' cabin, but they must not forget the crew's.

The advent of jet propulsion introduced fresh problems. Of the two types of jet aircraft now in service, his own impression was that the Meteor was much quieter than the average fighter, but the Vampire was of the same order of noisiness, although the noise was of a different, and probably less offensive, type. Whether this was simply because the engine was just behind the cock-

pit or because one Goblin actually made more noise than two Derwents, he would not care to say. Fortunately, as the lecturer pointed out, the high frequencies of which turbine noise mainly consisted were comparatively easy to damp out. In view of the very considerable noise, largely of low frequency, produced by the jet itself, it seemed advisable that no occupant of an aircraft should sit appreciably aft of the jet orifice(s).

J. A. C. WILLIAMS (Assoc. Fellow) *contributed*: The figures quoted for engine noise levels had been taken generally for hangar tests where correction was made for wall interference. Must allowance also be made for any appreciable attenuation because of the distance the engines would be installed away from the cabin? Again, it was probable that forward speed had an effect on the distribution of noise energy around the engine installed in a fast aircraft. Was any major benefit likely to accrue from taking account of this in design?

The aerodynamic noise of axial flow fans had been a hindrance to their wider adoption in the ventilating trade. Developments had taken place lately which reduced the noise level considerably. The rotating pressure field noise source had been reduced by making the aerodynamic centre of any particular blade follow a curved line. Applying this to airscrews it was probable that backswept blades such as those developed by the Germans might have reduced noise from the pressure field. Again, the main purpose of such blades seemed to have been the reduction of tip compressibility effects, which meant implicitly a reduction of the vortex noise source.

The introduction of laminar flow aerofoil sections should reduce the aerodynamic vortex noise on wings. With airscrews the same should obtain but to produce such sections accurately imposed a hard task on the manufacturer. The advent of all-wing aircraft would affect the aerodynamic noise position and would presumably alter the problem faced by the designer. The proper positioning of luggage compartments in such aircraft might go far to attenuating the noise of engines and thus effect a saving in sound proofing material.

MR. FLEMING'S REPLY

The Chairman: The question concerning noise when the speed of flight reached that of sound was one on which it was interesting to speculate. He thought the answer was that the crew would neither be deafened, nor would they be in perfect silence. Noise from a source at the rear of the machine, *e.g.*, a jet in the tail, should not reach the cabin; but noise from a source to the side could reach the fuselage farther to the rear, and having once entered the cabin, could travel forward through the air or through the cabin structure. There was the further possibility of structure-borne noise being transmitted directly from the engines to the cabin. Probably the major source of noise, however, would be aerodynamic. With laminar flow over the noise of the aircraft but turbulent flow over the rear part of the fuselage, there would be considerable noise entering the rear of the cabin, which would again be transmitted forwards inside the machine.

Dr. Still: There were many instruments on the market which measured sound in one way or another. But the objective measurement of a subjective characteristic of sound, such as loudness, was not by any means easy, and there was no instrument which was yet considered sufficiently satisfactory to be adopted as a standard for such objective measurements. Different meters, which perhaps attempted to simulate some of the characteristics of the ear in different ways, might not agree in their readings of the same noise, and it was possible also for the same meter to give readings on different noises which were in contradiction to those the ear heard.

For instance, the loudness of a noise consisting of a series of rapidly repeated pulses depended more on the peak value than upon the root mean square value, which latter was comparatively low on account of the silent periods between pulses. An instrument designed to measure the root mean square value would give a low reading on that type of noise, as compared with that heard by the ear, or as compared with the reading given by an instrument designed to read something near the peak value.

For this reason, among others, it was suggested that measurements of aircraft noise should be in terms of some objective characteristic, such as the overall physical intensity or, better still, the physical intensity in a series of frequency bands. To get the maximum information from the measurements the bands should be as narrow as possible, but the narrower the bands, the more difficult the measurements became, and the longer the period required to obtain a set of readings. For the general description of aircraft noises he felt that analysis into octave bands was sufficient, although for some purposes a finer analysis was desirable.

In interpreting such analyses it was important to bear in mind the limitations of the method, and the differences which could arise in analyses of the same noise by instruments using different widths of band. For example, in the analysis indicated by the broken line of Fig. 2 the points alone, representing the sound intensity in successive octave bands, had significance, and the line joining them indicated only that the successive points formed a single set of observations on the same noise. If a narrower band width were used the extent to which the two analyses would agree depended on the actual characteristics of the noise. Supposing that in a particular band the intensity was due to a single component, then with a narrower band analysis the same level would be obtained in the band containing the frequency concerned.

If, on the other hand, as was probably the case at the higher frequencies, there was a more uniform distribution of the sound energy with frequency, then the intensity in a band of one-third of an octave would be only one-third of that in an octave band, and the levels would be 5 db. lower. With a band width of one-tenth of an octave the levels would be 10 db. lower, and so on.

Another form of analyser might be had in which, instead of the band width being a constant fraction of an octave, it was a constant number of cycles per second. Then, as the frequency was increased the band width —perhaps 20 or 100 cycles per second— became a smaller and smaller proportion of one octave; and if the intensity per octave were constant, the intensity in such a band would be falling off by 3 db. per octave. So that in all types of analysis it was exceedingly important to specify just what was being measured.

Wing Commander Cave-Browne-Cave: He was present during the experiments with trainer aircraft which he had described and which had shown the value of screening the exhaust by placing it above the wing. Such measures should always be taken where possible. The main source of noise should be screened from the cabin; if manifolds were used, the orifices should be screened from the cabin by the engine or by the wings.

When he had stated, in introducing the paper, that there was no point in reducing one noise farther than about 6 db. below the level of the other noises he had intended to imply a noise at any particular frequency. Of course, if one noise were more annoying than another, it was valuable to decrease the first to a greater extent. But if there were another source of noise of the same frequency, there was no point in reducing the one farther than about 6 db. below the other.

He agreed that care must be exercised in interpreting the results of tests made in test houses; on the other hand, if no measure-

ments were made except in completely satisfactory conditions they would proceed much more slowly. Measurements in the test house did enable them to examine the variation of noise with operational conditions, and so on, much more easily than did measurements made in the air.

Mr. Morley: So far as the comments on Fig. 5 were concerned he would prefer to leave the matter to Dr. Still, as the measurements were made for Rolls-Royce Ltd. Dr. Still's remarks and the results he had obtained were extremely interesting, and he hoped they would be published with fuller details of the different types of exhaust system.

Mr. Morley's remarks on the difference between the noise of an airscrew in flight and on the ground emphasised the need for extending measurements, such as those of Fig. 7, to include the effect of forward speed. Possibly the example he quoted was to be explained by the fact that the noise depended upon the thrust and torque of the airscrew as well as upon the tip-speed. For a given blade angle and rotational speed the thrust fell with increasing forward speed, so that it was to be expected that the noise would be less in flight than on the ground, and would increase less rapidly with tip-speed.

Mr. Leach: He was inclined to agree with him that the recommended maxima of noise levels need not necessarily be changed because the jet engine might make a different type of noise from that of the reciprocating engine. The maxima should not be regarded however, as fixed for all time; if something better could be achieved, it should be achieved. Perhaps a little more high-frequency noise could be tolerated if the low frequencies were reduced; but this remained to be shown.

Mr. Makinson: His correlation between the results of Laird on the annoyance of pure tones and Postlethwaite's data on the annoyance of vibrations was very interesting, and his remarks on what was required for comfort on a long flight seemed very much to the point. Possibly laboratory experiments on the relative annoyance of noises having different types of spectra might assist in arriving at the most appropriate values for the recommended maximum levels, although there might be a danger that assessments of annoyance during observations over short periods might not accord with experience in flights of long duration.

Mr. Williams: The fundamental of the airscrew noise was the largest component at low tip-speeds only. At high tip-speeds higher harmonics became more prominent. There appeared to be no evidence suggesting a contribution from sub-harmonics. As was stated in the paper, the measurements of engine noise given in Fig. 5 were made at a distance of 8 ft. to the side of the engine, and the estimates of the noise from a jet were for a point 10 ft. from the jet orifice at an angle of 45° to the rear; that is, at distances of the order of those which might occur in an aeroplane. In high-speed flight the noise would tend to be radiated more to the rear than would be the case on the ground, and with the jet engine forward speed might result in a decrease of noise owing to the smaller velocity of the jet relative to the surrounding air.

Mr. Lee: The directional effect of noise from an aircraft might be due to a variety of causes. In the jet engine, for instance, the noise appeared to be produced along the boundary of the jet, rather than at the jet orifice, and was radiated to the sides more than directly to the rear. Part of the apparent directional effect of noise from a machine such as the Vampire might also be due to its high speed, and the fact that the noise reaching an observer on the ground came from a point some distance behind the aircraft.

Mr. Richards: His plea for more quantitative data was one which he also was

trying to put forward; he had tried to indicate the lines on which more quantitative data were required. He was also in entire agreement that noise should be considered in the project stage.

On aerodynamic noise no measurements appeared to have been attempted in regard to the factors on which the noise depended, how it was likely to vary with air speed and how it was likely to be affected by small projections, and so on. Such measurements might be difficult to make, there might be difficulties in devising a suitable method of test; but every effort should be made to find out more about aerodynamic noise. It seemed likely that, other things being equal, the noise intensity would increase more rapidly than as the square of the speed (suggested by Mr. Richards). Morley's results at speeds of 100 to 150 m.p.h. were more in accordance with an increase proportional to the sixth power of the speed.

THE ROYAL AERONAUTICAL SOCIETY

The 693rd lecture read before the Society

AERONAUTICS AND THE METALLURGIST

by

LESLIE AITCHISON, D.Met., B.Sc., F.R.I.C., F.R.Ae.S.

Professor Aitchison graduated from the Universities of Sheffield and London. He has had wide practical experience in the Aircraft and other industries having been consulting metallurgist to the Air Ministry, the Association of Drop Forgers and Stampers and various motor firms in the Birmingham district. In 1945 he was appointed to the newly-created chair of Industrial Metallurgy at the University of Birmingham. He was elected a Fellow of the Society in 1946.

A MEETING of the Royal Aeronautical Society was held in the Lecture Hall of the Institution of Civil Engineers, Great George Street, S.W.1, on Thursday 25th April 1946 at which a paper by Professor L. Aitchison entitled "Aeronautics and the Metallurgist" was presented and discussed. The President, Sir Frederick Handley Page, presided.

THE PRESIDENT : They were to have that evening a Lecture by Professor Leslie Aitchison. Professor Aitchison graduated from Sheffield and London Universities and became Lecturer in Metallurgy at Sheffield. From 1916 to 1919 he was Deputy Head of the Materials Section of the Air Ministry, and from 1919 to 1924 he was Consulting Metallurgist to many bodies, including the Air Ministry. In 1924 he was appointed Superintendent of James Booth & Company, which post he held until 1944. In 1945 he became Fellow in Industrial Metallurgy at Birmingham University and this year he became Professor in that subject. He was a Fellow of the Royal Aeronautical Society. He now had much pleasure in calling upon him to deliver his Lecture.

WHEN first I was approached to lecture to the Society it was suggested that I might speak to you on "Modern Aircraft Materials." I did not choose this subject, firstly because I felt it was much too comprehensive to be susceptible of compression within the period of any ordinary paper; and secondly, because the relevant data (in so far as it is known) is enshrined in the official specifications, in the catalogues published by the metal manufacturers, and in a large number of papers presented to this and other learned Societies.

Although I declined this project, I accepted the invitation to talk on materials because I believe the subject is of compelling importance to-day as never before, and is well worth ventilating on broad lines. Also, because I think an occasion when we may take stock and review the results thus revealed is extremely valuable both to the maker and to the user of aircraft materials. I shall therefore endeavour to take a broad survey of the subject, making the definite reservation that it shall be confined to metallic materials.

Rather than attempt a deep examination of any one portion of the subject, I propose to comment on a few aspects that are of broad interest. If I can do that from the point of view of the modern metallurgist I shall feel satisfied, and if I can stimulate some discussion of these topics, both by the engineers and the metallurgists, I shall feel the time is well spent.

I am sure there will be agreement that a moment falling close upon the termination of a great struggle that has occupied years filled not only with fierce physical strife, but with

the most intense mental activity, is an exceedingly appropriate time for stock-taking. During the war, problems had to be tackled with fierce resolution as they arose, and answers found as quickly as possible. Now that hostilities have ceased we may try to look round a little more generally, and seek for the more fundamental or basic problems, the solution to which are naturally the more rewarding in the long run.

This review is particularly appropriate in connection with aeronautical materials, since I would venture to assert that more metallurgical developments of recent years can be traced to the problems posed by aircraft and aero-engine designers, and the efforts made by the metallurgist to solve them, than to any other single cause. I would suggest, therefore, that in this brief review we approach the subject by the method of trying to reach an answer to three questions, namely, "Where have we come from?" "Where are we now?" and "Where are we going?"

It may not be difficult to find an answer to the first two, but the last might be, and probably would be, answered differently by various people. I can only put forward my own answer, and that without claiming any particularly prophetic sense or a cloak of infallibility.

Looking back over the thirty years during which I have been connected intimately with the metals used in aircraft, I cannot but be struck by certain broad conclusions that seem to be rather interesting. Of these, the two that strike me more forcibly are: first, that so far as simple strength, as displayed by the " maximum stress " is concerned, the materials used in this recent war differ but little from those used in the last; and second, that the aeronautical metals employed by all the protagonists, friendly and enemy alike, in the recent war are virtually identical.

If these two statements be combined and taken together they would suggest that over the whole world there has been a dearth of development and of progress, and that the activities of the metallurgical laboratories and industries of all civilised countries, though fertile in production, had been sterile of invention. This is not a true deduction but it may be worth while to see in what way, and to what extent, it is false.

Casting back to the position at the end of the War of 1914-18, one finds that, on the face of it, engine materials—especially the steels—were fairly highly developed, particularly on paper. The broad and general distribution of metals in and round the piston engine was not vastly different then from what we find now. In the airframe, however, things were very different.

The Kaiser's war was waged to a preponderating extent on wooden machines, and the metals employed in structures were neither varied in type, nor extensive in quantity. Mild steel was the principal metal, and other than this simple commodity the airframe comprised some high-tensile wires (streamline and otherwise) with their attendant fittings, and a little light alloy, often used in an apologetic and furtive manner. For some purposes trifling quantities of alloy steel sheet were incorporated, and "high tensile" nuts and bolts found a certain measure of favour. Nevertheless, again on paper, all these materials had good mechanical properties, and in the intervening years we do not seem to have moved a very long way forward, if we judge by specification requirements.

On viewing the matter on the strength basis it seems not far wrong to say that steel, with a high level of maximum stress at 100 tons per square inch, was as strong in the last war as in this; and that the wrought light alloys, at 25 tons per square inch, in 1918, were only from 3 to 7 tons per square inch, depending on the manufactured form, weaker than the materials of 1943. And that does not seem much to show as the record of the years between the wars and as the result of the millions of pounds expended.

This very simple statement may suffice to indicate where we came from, but it is worth while to dig a little deeper and to see what

really has happened during the isthmus years, with the intention of reaching an answer to the question: "Where are we now?" This may be stated best by indicating what seem to be outstanding developments of the period, taking them field by field.

Before scheduling the advances that reflect themselves in the specifications, it may be pertinent to make some mention of the complete revolution that has overtaken metallurgical thought and theory as a result of the application of X-ray analysis and the science of crystallography, together with the electron theory of matter to the study of the metallic state. To-day it seems difficult to realise that the Kaiser's war was fought and won before the work of Sir William Bragg and his son was carried to the stage at which it was employed usefully in the study of metals. But it is undoubtedly true that from 1920 onwards, our ideas of the metallic state have been modified, extended, and corrected by the application of this particular technique.

Through this knowledge we possess a conception of metals and alloys totally different and infinitely more fertile in possibilities than was possible when our "sight" of metals was limited by the capacity of the ordinary optical microscope, however powerful. Through X-ray analysis we are able to discover many things previously hidden, and this not only in the field of metallic structures as such, but in the realm of stress and strain distribution. Further, and perhaps of the greatest ultimate interest, the investigation of the structure of metals, based on crystallography, the electron theory of matter, and thermodynamics, is yielding generalisations of a most illuminating type that will undoubtedly affect metallurgical development most markedly in the years to come.

For the first time, the science of metals has become susceptible of a mathematical treatment, which virtually means that it has passed from the empirical to the exact. This, then, is the atmosphere in which the new theory of metals has emerged and developed during the past twenty years or so, and which undoubtedly will guide and govern all future fundamental progress.

Turning from the theoretical or fundamental to the more obvious, certain developments stand out as the fruits of the activities of the isthmus years. In the ferrous world, two, not entirely unrelated, leap to the mind. The first is the blossoming of the stainless steels in their abundance and variety.

During the Kaiser's war only one kind of stainless steel was in serious use, that being virtually the original form as generally used for cutlery, and which, be it remembered, was only developed successfully about the year 1914. This steel, used most particularly in engines as one of the materials for exhaust valves, found but little general application in aircraft. Its particular virtue could not be utilised extensively in engines, and in air frames the material could be employed only for forged or turned parts, the metal not being available for structural members of the usual type.

A more comfortable version of the same kind of steel (the 12 per cent. chromium type) but with a much lower carbon content, came forward just after the old war and was called "stainless iron." This modification again did not quite fit in with aircraft requirements. It required a radical change of type in the stainless alloys, resulting in the production of a new group composed of iron, chromium and nickel with small contents of carbon, to provide stainless steels suitable for general fabrication and incorporation in aircraft. The steels in this class are well known to-day and have many virtues. But it must be noted here that their supreme virtue is their resistance to corrosion and that they have not extended in any way the range of mechanical strength available to the aeronautical engineer.

The other ferrous development, faintly related to the stainless steels, is that of materials to give satisfactory service at high temperatures. This problem has always been of some importance in aero-engines but has come, and will come, into greater and even greater prominence in the evolution of the jet

engine and the gas turbine. The problems of strength and permanence at high temperatures are rather special ones, but the materials put forward so far to meet the demands are perhaps rather less unusual than might have been expected. In view of this conventionality, which exists despite the large efforts to break it, it is not surprising that here again the materials developed for high temperature service, notable and remarkable though they have been in their own field, have had little, if any, effect on the mechanical strengths that the aeronautical engineer can call on for structural purposes.

Perhaps the non-ferrous metals have displayed rather more spectacular developments, which may partly be related to their vastly increased employment, particularly in airframes. This increased use is most notable in the case of the so-called light metals, and the development of this interesting group of materials may be worth examination in some detail. The fight for higher values of mechanical strength has been fought more fiercely in this field than in any other. The group must be sub-divided and lends itself to this process quite naturally, the sub-groups being (*a*) the wrought alloys of aluminium; (*b*) the cast alloys of aluminium; (*c*) the wrought alloys of magnesium; and (*d*) the cast alloys of magnesium.

In regard to the wrought aluminium alloys it has already been mentioned that the Kaiser's war ended with Duralumin—the old standard form "B"—as the strongest material in the field, the alloy having a specifiable maximum stress of 25 tons per square inch (26 tons in tubes). Proof stresses were not fashionable statements in those days, but the alloys possessed a proof stress of 15 tons per square inch, even if it was not written that way. This alloy, Duralumin B, is preeminently the alloy of aluminium that hardens by ageing at room temperatures, after quenching from the appropriate high temperature, and it is not without interest to note that twenty-five years of research and development by all and sundry has failed to produce an alloy that, as a consequence of room temperature ageing, possesses any appreciably higher values of proof stress and maximum stress.

A separate line of development has been pursued both intensively and fruitfully with alloys of this type, namely the application of precipitation heat treatment, which is simply the old ageing carried out at temperatures higher than atmospheric. Many of the alloys that respond most usefully to this particular treatment do not differ very markedly in composition from Duralumin B, and it is convenient to regard them all as members of the same family. The metallurgical behaviour of them all is based on the reactions of magnesium silicide and the intermetallic compound of copper and aluminium, added to the parent metal.

The small modifications of composition have led to alloys that, when quenched and then aged at temperatures of the order of 175°C., possess rather higher values of maximum stress than are found in Duralumin B and considerably higher values of proof stress. The maximum stress of Duralumin has been lifted by these means by only about 5 tons per square inch, in the most favourable circumstances, but its proof stress has risen from 15 to 26 or 28 tons per square inch.

Probably this is the most important of the obvious developments of aeronautical materials achieved in the isthmus years. It suggests that the development of the aluminium, magnesium-silicide, and $CuAl_2$ series of alloys has been pushed about as far as it is likely to go with conventional methods.

A departure from the "Duralumin" system is found in the alloys that contain a fair content of zinc—5 to 7 per cent—together with copper, magnesium, silicon, and manganese in appropriate proportions. This particular combination has probably not yet been worked out to its conclusion, or to the best results, but at any rate it has opened out quite a new vista and has lifted the strength

properties of the wrought aluminium alloys to figures approximating to 35 tons per square inch for the proof stress and 40 tons maximum stress.

In the field of the cast aluminium alloys the changes, as reflected in the available mechanical properties, are quite as striking. At the time of Versailles, the usable casting materials were the old L.5, L.8, and L.11 alloys, the highest strength available from them being about 10 tons per square inch. In those days little heed was paid to anything in the nature of a proof stress (or even a yield point) in a casting alloy, but the value corresponding to the 10 tons maximum stress may be taken to be about 4 tons per square inch.

Two lines of development were initiated about that time, one line proceeding from "Y" alloy as developed at the National Physical Laboratory, and the other through alloys of aluminium containing round about 12 per cent. of silicon. Each of these developments was a radical departure from the straight (or nearly straight) alloys of aluminium with copper or zinc, and greatly widened the useful compositions of the casting alloys. The former material, "Y" alloy, deriving as it did from the same primary stock as the Duralumins, naturally led to the application of heat treatment to aluminium castings, with the specific intention of improving their strength properties.

This idea bore fruit and was later applied even to alloys based on the 12 per cent. silicon mixture, the net result from all sources being that whereas in 1919 the strength properties of the cast alloys were about 4 tons per square inch, for the proof stress, and about 10 tons for the maximum stress, in 1945 the values stood at 16 and 18 tons per square inch respectively. In round figures, therefore, the cast alloys may be said to have improved rather more than the wrought alloys, during the isthmus years.

A consideration of the development of magnesium alloys is difficult because of the lack of a basis for comparison with the old war years. At that time no magnesium alloys were in use, although even then there was some embryonic activity. We can only, therefore, look at this metal on the basis of "where we are," and neglect any attempt to deal with "where we came from."

All kinds of comparisons are made between aluminium and magnesium alloys, but they are usually complicated by the fact that although the density of magnesium is but two-thirds that of aluminium, its modulus of elasticity is also two-thirds that of the heavier metal. Therefore, we should think primarily, and, at this moment, solely, of the actual strength properties that have emerged as the result of an enormous volume of painstaking research and conscientious development.

Logically, castings should be considered first as they usually come first in the history of development. In this field the present position is that one may use magnesium alloys either "as cast," or quenched, or quenched and precipitated on the same lines as the aluminium alloys. The best strength values any of these forms will yield, as specified, are a proof stress and a maximum stress of about 5.5 and 15 tons per square inch respectively.

On the wrought side the magnesium alloys have developed considerably, but the fabrication difficulties have resulted in a considerable variation in the properties found in the different forms in which the materials are used. For our present purpose this variation may be ignored because our aim is to see what mechanical properties the alloys, as such, can be relied upon to provide, in the best form. On the basis of the projected specifications the highest combination of strength properties is found to be approximately a proof stress of 17 tons per square inch with a maximum stress of 23 tons.

The other more obvious developments in the non-ferrous sphere are twofold. The first is the application of the process of full heat treatment—comprising both solution and precipitation treatment—to certain copper alloys. Several groups of alloys have appeared during the period under review that owe their useful mechanical properties to this form of

TABLE 1

Material	Proof Stress tons/sq. in.	Max. Stress tons/sq. in.	Elongation %	$E \times 10^6$ lb./sq. in.	Density	Max. Stress / Density
Medium, hardened and tempered steel	55	70	16	30	7.8	8.97
Highest strength structural steel		100	12	30	7.8	12.8
Beryllium Bronze	65	85	5	18	8.2	10.35
Cast Aluminium alloy	16	18		10	2.8	6.43
Cast Magnesium alloy	4.5	15	10	6.5	1.8	8.3
Wrought Aluminium alloy	28	32	8	10	2.8	11.4
Wrought Aluminium alloy, projected	35	40	5 or less	10	2.9	13.8
Wrought Magnesium alloy, projected	17	23	8	6.5	1.8	12.8

treatment, the most remarkable being that of copper with small percentages of beryllium—the so-called "beryllium bronzes."

These alloys respond to mechanical and thermal treatments very satisfactorily and the resulting strength figures are striking. By heat treating an alloy of copper containing about $2\frac{1}{2}$ per cent. of beryllium, a combination of 65 tons per square inch proof stress with 85 tons per square inch maximum stress can be secured.

The remaining non-ferrous development is that of alloys prepared specially to possess useful mechanical properties at high temperatures. These alloys are various in composition and rather specific in their application, the most conspicuously successful of them being those based on nickel. Like their ferrous counterparts they are admirable in their special field, but do not carry us much farther in the search for stronger and ever stronger structural materials.

Before leaving this brief review of the obvious developments that have taken place during the isthmus years it may be worth while to summarise the position in Table 1, by stating the values of the principal properties for the leading materials as they stand to-day. This table, in brief, gives some kind of picture of "where we are now," and although it is a tabloid it serves a useful purpose in providing a basis for the consideration of "where are we going?" especially if this is to have particular regard to the strength properties of aeronautical materials.

The developments dealt with so far are those that "he who runs may read." But if the statement were left at that, a false picture might be formed. The less obvious side of the picture is just as important as the more obvious, but is not appreciated so readily, or else is taken for granted. The improvements that are not so easily observed have played an enormous part in the betterment of aircraft design and performance, and have contributed particularly to a reduction in the factor of ignorance, or the allowance for uncertainty, in the calculations.

These unspectacular developments do not appear in the specifications or the records, but have been notable in transforming materials of doubtful reliability into metals in which the designer can place reasonably full confidence. There is little doubt that many times more effort has been put into the provision of these "intangible" properties than into the development of those properties that can be written into the material specifications.

The qualities that have yielded to this improvement may probably be stated fairly readily. The list would contain such headings as "soundness," "purity," "uniformity," "workability," "homogeneity," "reproducibility," and "quality." (To these might be added "permanence," though that quality is not quite in the same category as the others mentioned.)

A great deal might be written under each of these headings, were there adequate time for such a discussion. It remains true, however, that the materials of 1946 are exceedingly superior to those of 1918 in regard to all these qualities, and that the modern aeronautical engineer is much more comfortably placed than was his predecessor twenty-six years ago. To-day he can have some confidence that the metal he is using will be what it is represented to be, and, further, that it will be up to his expectations throughout the whole of the quantity in his stores. The fact that some of it may even be better than he expects is a bonus due to the developments mentioned and is an aspect of the matter to which we may return later.

If what has been said so far represents truly, though briefly, the position in which we are now, it should serve as a jumping-off ground for the consideration of the vitally important question "where are we going?" What has been done is naturally a matter of fact. What the course of future development may be is equally necessarily one of surmise and opinion. It may well be a very large subject and to make it more reasonable it may be desirable to spend a little time in marking out some of the aspects meet for consideration. Obviously no one can lay down rules for future development, or forecast the results of research. It is only reasonable to try to pose the problems as intelligently as may be, in the hope that future investigations will be directed to those matters that one feels may be really rewarding.

In a discussion of this kind it seems worth while to mention that the subject will be almost certainly regarded differently by the engineer and the metallurgist. Since the engineer is "the customer" and therefore proverbially "always right," it is important that the metallurgist should try to appreciate his point of view. I hope I shall succeed in making a reasonably accurate statement, because it is essential for the metallurgist to grasp the engineering point of view if he is to be helpful. In respect of aeronautical metals it is the engineer who states the ends and the metallurgist who must contrive the means, if he can. What, then, are the ends devised by the engineer?

At the risk of being unduly brief, I think they may be stated simply to be the possession of the highest possible mechanical properties by metals of the lowest possible density that are at the same time homogeneous and permanent. The term "mechanical properties" must not be interpreted too narrowly. In addition to the strength indicators, proof stress, maximum stress, fatigue strength and creep resistance, the elastic moduli have to be regarded, and finally the engineer pays a good deal of attention to ductility.

Thus interpreted, the statement seems to be sound, though it must not be forgotten that the development of aircraft, and particularly of the various types of power unit, has widened material requirements in the sense that the properties of metals at temperatures substantially below, and very considerably above, atmospheric are now of the greatest importance. However, for present purposes, the simple statement may serve not inadequately.

These being the ends, what of the means? As has been mentioned already, the metallurgist of to-day regards his problems from rather a different viewpoint than formerly, and because of his greater and deeper knowledge of the metallic state, is, perhaps, better able to see the wood without losing himself in a contemplation of the trees. Possibly this may be what he sees.

It has already been mentioned that the strongest steels used for structural or constructional purposes are those with a specified strength, *i.e.*, maximum stress, of not more than 100 tons per square inch. (Admittedly, this excludes, and rightly, such things as cold drawn wire.) It has been shown, too, that the strongest alloys of aluminium at present specifiable have a strength of about 40 tons per square inch, those of copper about 85 tons, and those of magnesium a value of the order of 23 tons.

If these figures be compared with the strength of the parent metal it emerges that the structurally usable steels have a strength about five times greater than that of iron, the corresponding aluminium alloys a strength from nine to ten times that of the unalloyed metal, the stronger copper alloys a strength about six times that of copper, whilst in magnesium the increase has been approximately three times.

So far as I know there is no law that lays down the greatest possible magnification in strength of a metallic element, but a broad view of the facts of the history of alloy development seems to suggest that in the case of iron, aluminium, and copper, at any rate, the limit of strengthening through the conventional and explored processes of alloying and heat treatment has been approached. This view is based on the strength values actually used, but is not even approximately true if strength, and strength alone, be taken as the sole criterion.

In the previous paragraphs I have adhered to "specification" figures, and these have reference only to what I have called "structurally usable" materials. If this limitation is forsaken the strength values attainable by alloying and heat treatment are manifestly much higher. Among the ferrous metals, consider, for instance, the values possessed by such articles as a razor blade, a bearing ball, a fully-hardened high speed tool. All these have strengths far and away higher than 100 tons per square inch and each in its place functions perfectly well, but these functions are not structural. Similarly, among the aluminium alloys the present strengths represented by maximum stress values of 28 to 40 tons per square inch can be raised considerably, but in the process there is a great sacrifice of ductility, the resulting material becoming more nearly akin to the razor or the ball bearing, namely strong but brittle.

To what extent are the engineer and the metallurgist justifiably held in thrall by ductility requirements, not merely those of specification but of the tradition in which we all were nurtured? Is the insistence on "elongation" the worship of a fetish, the fear of a bogey? The point seems to be worth examination. Of course it always has been, but when we are rather grounded in our efforts to produce stronger metals by the demand for a high value of "elongation" the examination of the problem seems particularly necessary. A full consideration requires at least a paper devoted solely to that topic, with a fully-representative discussion, but a few aspects of the subject may be set out briefly.

Firstly, it will be agreed generally that the recognised measure of ductility—elongation—has played in the past an important role by acting as a safeguard against the employment of materials of inferior quality, produced as a result of imperfect handling at some stage of manufacture. This is no mean achievement, and it is very probable that elongation owes its continued popularity, or its survival value, more to this service than to anything else.

Secondly, the possession of a good measure of elongation is generally regarded as a helpful or even an essential attribute of metals that have to be shaped cold, by pressing, spinning, bending, or the like process. Faith in elonga-

tion as an infallible guide to workability has been somewhat frowned on recently, but some reasonable endowment of ductility seems an inevitable requirement in materials destined to be submitted to cold distortion.

Thirdly, metals must be ductile, or "possess elongation," in order that they may flow at places of high stress concentration and so, by a redistribution around the stress raiser, remove the danger that the local stresses will rise to a dangerous or fatal value. Usually this particular action is regarded as the most critical of all the reasons that can be advanced for demanding a high degree of ductility in a structural material, but there may be some doubt whether it is quite such a valid argument as it appears to be. Quite possibly the actual flow that takes place is much less than is usually imagined and considerably less than that represented by the stipulated value for elongation. Careful research as to the actual flow required to achieve stress relief would be most rewarding.

Against the points so far mentioned it is reasonable to remark that castings are used in conditions of fairly high stress quite regularly and successfully, these castings frequently having very low elongation values indeed. It may also be worthy of notice that the elongation figure in a purchasing specification, and that normally recorded in the tensile test, represents the total flow of the material up to the point of actual severance. Such a value can have but little relation to the behaviour of the metal in conditions of actual service, or even in fabricating processes. Only a small proportion of the plastic flow recorded in the tensile test is ever called into play in service.

This crude popular measure seems therefore to be singularly unhelpful in revealing the properties required by the structural engineer, an opinion that is rather strengthened than otherwise by recalling, firstly, that the normal fatigue value of a metal, determined by the orthodox method, is not influenced by variations in its ductility; and secondly, that in resistance to creep, a material in the cast form —and therefore possessing an inferior endowment of elongation—is often superior to the same material as wrought and therefore showing much higher values of elongation.

I might adduce the impressions gleaned from an extensive experience in the examination of parts that have failed in service. If elongation was an attribute as important as it is often thought to be, the history of aircraft failures might well be littered with the records of parts that have come to grief as a result of a deficiency in that property. Speaking for myself, and after looking into the matter again, I cannot recall a single example of a service failure that can be attributed solely, or even primarily, to insufficient ductility in the material.

Insistence on a "high ductility" value in materials, whether rightly or wrongly, is undoubtedly acting as a bar, or at least a hindrance, to the realisation of higher strength values. It seems worth while to come to grips with the problem and to separate the ductility utilised during fabrication from that required by the metal in order that it shall function satisfactorily in service.

Certain aircraft parts are made from metals produced in forms that require little, if any, subsequent fabrication by the aircraft constructor, such as castings, forgings or extruded sections. These forms would appear to lend themselves to experiments of this nature, and might incidentally be adapted also to reduce the number of mechanical joints, which are such a cramping feature to many designers. I have no fear of insuperable difficulties being encountered in efforts to devise acceptance tests, designed to discover inferiority in manufacturing methods, that could replace the present elongation value in the specifications.

Possibly the problem can be put simply by asking whether it is always necessary to use a metal for structural purposes that has a stress/strain curve of the type given in Fig. Ia. This has all the virtues that have been looked for in the past and a metal of this characteristic will be capable of undergoing a great deal of plastic strain before fracture. The engineer does not want and could not

endure much plastic strain in his structure. A metal with no elongation at all would have a curve of the type shown in Fig. Ib, and I venture to suggest that quite a lot of useful engineering materials approximate to that. But is not a metal giving a stress/strain curve of the form shown in Fig. Ic quite conceivably valuable?

The matter of ductility may also have a profound effect on the practical possibilities of raising the value of the elastic moduli in materials of construction. The increased knowledge of the metal state, and the physical theories applicable to it, have shown that there is a possibility of bringing about useful changes in the values for the moduli. Doubtless the aeronautical engineer would benefit enormously if materials having the same order of strength and density as those at present in use, but possessing higher values of the elastic moduli, were available.

The metallurgist has so far probed but little into this field, and his probings have usually been cut short by the requirement of a fair endowment of ductility in the product. This is another example of the supreme importance of investigating the ductility problem most carefully and thoroughly. The question "what is the least value of elongation you really require?" has often been put to engineers, but the answer has usually been, quite frankly, "I don't know." It seems that now is the time to find out.

The same consideration arises—that of the minimum acceptable ductility—when efforts are made to increase the ratio of proof stress and maximum stress in the alloys responding to precipitation heat treatment. The curves of change of properties for those materials follow the general form shown in Fig 2. They indicate that increased precipitation raises the ratio between the proof stress and the

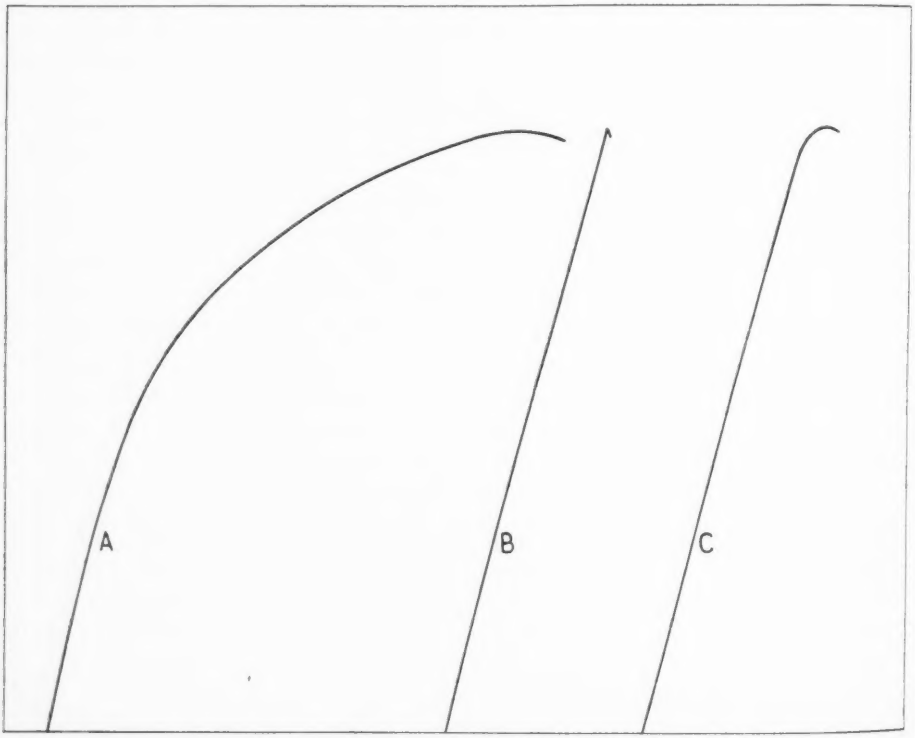


Fig. 1

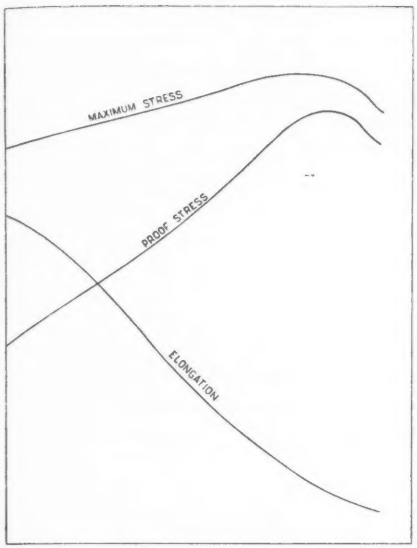


Fig. 2

maximum stress, but that concurrently the elongation value falls away.

A high proof stress ratio is one of the dearest wishes of the aeronautical engineer, and usually he has put up with lower values of elongation to acquire it, possibly without realising that he was doing so. He may now think it worth while to go still farther and to get down to rock bottom for the ductility values. If he does, he may well reap a good dividend in proof stress ratio as well as in other directions.

A consideration of the minimum usable ductility is of importance in connection with metals in all sorts of parts—even in materials designed to serve at high temperature. In the alloys intended for use at atmospheric temperature or thereabouts, these being the principal concern of this lecture, any process that forces a metal to give the utmost strength values of which it is capable, and this is particularly true of precipitation heat treatment, seems to bring with it an increased susceptibility to corrosion, particularly of the intercrystalline type. Whether it is equally true that resistance to stress corrosion is likewise diminished under these conditions remains to be proved, but it is a possibility. These considerations need not be fatal objections to any strength-raising efforts, since corrosion difficulties are usually—in the ultimate end— controllable by suitable protection methods.

In addition to strength and rigidity in metals used at and about atmospheric temperatures, the metallurgist is now being asked for metals with valuable properties over a very wide range of temperature. High altitude flying brings low temperatures; gas turbines and jet engines bring very high temperatures, while supersonic flight may bring moderately high temperatures even in structures.

In the materials that have to operate at super-normal temperatures, and still be light metals in the ordinary sense of the term, the problems of creep and resistance to fatigue are paramount. In respect of the former property we possess a good deal of information respecting the values given by many different materials under conditions arbitrarily defined, but we do not yet know the fundamental principles that govern the behaviour of metals in creep. The examination of the mechanism of plastic flow in minute amounts and the explanation of creep phenomena is no easy problem, but it must be carried through and understood completely if we are ever to be able to predict and classify our materials in regard to their creep resistance.

The problem of fatigue is perhaps not quite so difficult, if examined in the simplest conditions of loading and of shape of article. Unfortunately, these conditions are rarely found in aircraft. The effect of notches may perhaps be predicted, though possibly only qualitatively, but a great deal has yet to be discovered of the effects produced by fluctuating stresses superimposed on steady ones, or of the resistance offered to fluctuating stress cycles of medium magnitude into which are injected a small proportion of stress cycles of much greater magnitude. These are investigations applicable to all metals, and are fundamental work that ought to anticipate any attempt to side-track them, dictated by

the hope of the evolution or emergence of a metal or metals possessing properties that will justify the postponement of basic work to another day.

It seems unlikely that any metallurgical panacea for all these ills will be evolved. In the days of few metals, and those simple and straightforward to use, each of them had to be a kind of metallurgical "universal aunt." To-day there is a lingering feeling that because a metal is good in one direction, or has an attractive attribute that renders it excellent for certain purposes, it might well be employed for many other purposes.

Fortunately this feeling is dying. It is now more generally recognised that for any particular purpose one metal (or alloy) is preeminently the most suitable and should be employed. It is inevitable that every material eventually finds its own level. No amount of special pleading will bring it into general use, unless such employment is justified by its properties. To-day we may have to go even farther in the direction of specialising than we have hitherto attempted.

That quite low values of ductility might be perfectly serviceable in some types of part has already been suggested. For some services the proper materials must have the properties indicated by Fig. 1a. In another form Fig. 1c might well suffice. But the difference between the two materials may only be one of state and not of composition. Again, it might well be that the metal depicted in Fig. 1c behaved admirably in compression members but that in tension the state corresponding to Fig. 1a must be employed. I think the metallurgist to-day could give both the forms, and the engineer could fit them to his requirements. It is not too much to hope that the era now opening may see the development of metals fitted to the requirements, the reverse of what happens now when the metal is specified always in terms of one set of values—those revealed by the conventional tensile test.

It is well recognised that the engineer, in every branch of his profession, is bound to think in terms of physical or mechanical properties. He is not a bit interested in the means the metallurgist employs to produce those properties, but I think he may be interested enough to endure a brief review of the prospects of being offered metals with better properties than he has so far employed. This term "better properties" ought to be interpreted widely because of the specialised requirements of so many components of an aircraft or power unit.

If we were to examine every metallic property that interests the engineer the disquisition would be endless. We must, therefore, confine ourselves roughly to the conventionally accepted mechanical properties of metals possessing a density that makes them usable in aircraft.

For such a purpose the pure metals are obviously out of the hunt and all attention must be focussed on alloys. It seems to have emerged, in brief, that the only alloying systems that so far have resulted in anything that possesses properties useful to the structural engineer are those based on:

(*a*) iron; iron-carbide;
(*b*) aluminium;
(*c*) magnesium.

For certain special purposes the systems based on copper and on nickel are useful but for sheer mechanical value the three mentioned really cover the field. If the situation relating to these three systems is examined it is found that the alloying systems have been worked over pretty completely.

In the case of iron: iron carbide, the ternary systems produced by adding a third element have been examined exhaustively, while quaternary and quinternary alloys are almost as common as flies. But such alloys have not resulted in any striking improvement of the leading mechanical properties over those available thirty years ago. That they result in the evolution of other useful properties is clearly demonstrated by the development of the iron, carbon, nickel, chromium alloys of high corrosion resistance; of special magnets; and of superior valve steels.

Similarly in the domain of alloys to serve at elevated temperatures striking results have emerged. The alloys do not, however, yield higher strengths or higher values of the elastic moduli. In the case of aluminium alloys it is fairly safe to say that a survey has been made of all the probably useful binary systems and a good many of the ternary systems, too. Both with aluminium and magnesium it is obvious that interest is bound to be focussed on the alloys with other elements of reasonably low density. If startling mechanical results arose from alloying with the heavier metals the alloys would be acceptable, but a large increase in specific gravity unaccompanied by a very large increase in "strength" would probably mean a useless material for aeronautical purposes.

To make a bold statement that the limits of strengthening have been reached with these principal basic metals would be foolish, but it seems likely that it has been approached. It is also true that virtually all metals present in large quantities (from the metallurgical standpoint) in the earth's crust have by now been brought under examination and submitted to the traditional processes.

There is one metal that seems worthy of rather more attention than it has received so far, because it possesses certain properties that would endear it to the aeronautical engineer if some of its other properties could be smoothed out. Beryllium has two useful properties, at least, a low density (1.84) and a high value of Youngs Modulus (42,000,000 lb. per sq. in.). In other words its density is as low as that of magnesium while its value of Young's Modulus is considerably higher than that of steel.

A metal with this combination of properties is bound to be attractive to the aeronautical engineer if it can be made available. So far, it has not been produced in usable forms for aircraft. It is quite a common constituent (in low proportions) of heavy alloys—such as Beryllium-Bronze—but these materials do not interest the structural engineer. The metal itself—or an alloy or alloys very rich in beryllium—is the material at which to aim.

Beryllium is not terribly scarce, though possibly scarce enough to make it unlikely to serve as a wholesale replacement for the alloys based on aluminium and magnesium. It is stated to be about as plentiful as tin, which is hardly a rarity. So far, beryllium appears to be a very brittle material; whether it is essentially so, or whether the almost complete lack of ductility is the result of incomplete purity or contamination from the manufacturing methods employed has not yet been resolved. But alloys of beryllium with aluminium appear to possess a fair measure of ductility as well as some other useful properties. In table 2, some selected best results on such alloys are displayed. To say the least, this metal, and alloys containing considerable proportions of it, in the light of the very high values of modulus of elasticity thus attainable, are worthy of intense investigation.

Viewed from the traditional angle, and judging by the trends that have been summarised so briefly in the preceding paragraphs, it would seem that the outlook in regard to the possibility of producing stronger materials is rather dim. Just at this time,

TABLE 2

ALUMINIUM BERYLLIUM ALLOYS

BERYLLIUM %	FORM	DENSITY	MAX. STRESS TONS/SQ. IN.	ELONGATION %	$E \times 10^6$ LB./SQ. IN.
40	Wrought	2.2	18.5	14.5	21
35	,,	2.3	31.5	5	19
25	,,	2.35	15	18	17
10	,,	2.55	9.5	25	13

such a conclusion, if final, would be tragic, for the demands of the engineer for better materials to use in structure or power plants are perhaps more clamant than they have ever been.

For air frames, on the one hand, alloys with a higher ratio of strength to weight and possessing an increased rigidity are insistently demanded, while on the other we are assured by those competent to judge that the whole future destiny of the jet and the turbine engines lies in the hand of the metallurgist. There must be a response to these demands, and I think the attack on the problem may come from two directions—one of them perhaps by means of a long term approach, but the other susceptible of immediate action.

Earlier in this lecture I mentioned that the isthmus years witnessed a revolution in metallurgical thought and in the theory of the metal state dating from the advent of X-ray crystallography, but eventually based on the electronic theory of matter. The advances in our knowledge of the metallic state, arising from the discoveries relating to the constitution and structure of matter, have intensified metallurgical research in the physical and physico-chemical fields, and sharpened our appreciation of the behaviour of metals under a considerable variety of conditions. Concurrently they have provided some knowledge of the factors that must be taken into account when any attempt is made to predict the behaviour of the metallic elements in regard to such matters as mutual solution, and the formation of intermetallic compounds.

What was previously a collection of isolated facts is now beginning to fall into a pattern which may possibly reveal the rules that determine the behaviour of metals in this physico-chemical sense. On the more purely physical side, the new technique provides a weapon that enables the behaviour of metals under simple or combined stress, in fatigue, in creep, in plastic flow, to be investigated as never before, and permits fundamental knowledge on strain relations that no previous technical method could do more than suggest.

For a long time we have had formulæ resulting from the mathematical treatment of elasticity, which have been the engineer's statement of the behaviour of isotropic metallic masses. The new knowledge is permitting the mathematical treatment of the behaviour of the metal crystal itself, applying physical and thermodynamical methods to this most intimate problem.

Applied to the historic metals and alloys produced in the traditional way, when used conventionally, this new knowledge may not appear to be especially fruitful—except in providing sound explanations for many puzzling phenomena that have been regarded previously rather as "acts of God." But the new knowledge ought not to be allowed to be confined to such a service. It should be employed to extract new possibilities in metallic alloys and to indicate methods by which the newly-discovered properties of the older alloys may be employed to the best advantage. That this may involve a departure from tradition in regard to methods of production —or even of application—ought not to deter us.

This method of attack may not bring startling or immediate results, and is definitely a long term project. Its probable value lies in its approach to the fundamental properties of the solid state—as revealed in metals particularly—and the consequent revelation of the principles on which metals and alloys can be built and in terms of which their behaviour can be explained. It is well known, for instance, that the developed strength of the metals we use are far below those they might be expected to possess on the basis of their atomic structure. There is no likelihood of this difference being bridged—even in part—unless the reason for the difference is understood and explained.

It has been revealed by crystallographers and physicists that the arrangement of the metallic atoms within their particular crystalline lattice means that, in most metals, the elastic moduli differ according to the direction in which the stress is applied relative to the

axes of the crystal. This directionality of properties (which is not confined to the elastic moduli) has been used already in certain forms of engineering, and might possibly repay investigation in the direction of producing directionalised alloys for use in aircraft. Likewise, with regard to the vexed problem of ductility, and particularly in reference to its operation in the vicinity of stress raisers—and in fact in relation to joints in general—the application of the modern technique of investigation will undoubtedly be rewarding. Results cannot be promised but it is a fair bet that they will come.

Meanwhile there are certain "short term" considerations, the investigation of which can be expected to yield dividends fairly quickly. I hardly think they are revolutionary, except perhaps in the outlook required to put them into practice. It can be said without much fear of contradiction, that whereas aircraft metals have progressed from the fair to the good, they have not yet passed into the region of the superb. There are a number of reasons to account for this halting, but I can only suggest a few of them.

At the head of the list one must naturally put ignorance—or, shall I say, insufficient knowledge—of the way to solve the problems involved in the production of the perfect metal. That insufficiency is gradually being overcome, and when the process is complete will presumably solve the whole matter. In the meantime there are other operative reasons considerably connected with the conditions of material supply.

Except in time of war, and then only in certain sections, the demand for aircraft metals is insufficient to occupy the whole attention of the producing metallurgical industries. As a result, aircraft materials are produced alongside metals destined for less arduous service, or those that permit of a lower standard of excellence; are produced by the same personnel; in the same plant; and from virtually the same raw materials. All the metal so produced may be of good quality, but perhaps none of it will be of the very best. In this case the adage that " the best is the enemy of the good " must simply be reversed. The conditions of supply of aircraft metals must justify special production if the materials are to be of the quality needed.

I would not like it to be thought that I am suggesting the establishment of isolated production units from which the profit or the competitive motive is absent. I believe in competition, but I do not believe the greatest advances in aeronautical metals will be obtained at the present time if there is too great an insistence on the price factor and all that it entails.

I suggest, too, that the aeronautical engineer might have the courage to say that he is prepared to pay for a proportion—the best—selected from the bulk of production. Now that the war is over we have passed the stage when every ounce of metal coming from the forge, the press shop, the rolling mill or the foundry, and that scrapes through the specification requirements, must be used and built into aircraft. One result of a policy of this kind is that purchasing specifications will always be pitched a little lower than they really might be, to avoid excessive rejections and therefore actual loss of production. The consequence to the engineer is that though he has to design to specification values "by statute," he knows that a noticeable proportion of the metal he is using possesses mechanical properties considerably in excess of those printed in the specifications.

It might be argued that the right step to take would be to write up the figures in the specification. I suggest that such a step would be premature as I think it might not produce quite the result desired. I believe the effect of such action might be that every effort would again be made to force all the metal produced through the specification. This overforcing, say by heat treatments bordering on the dangerous, does not produce the metal the aircraft engineer wants.

For really efficient service in aircraft I believe the metal must be " comfortable,"

and it is that proportion of the production that possesses superior values as a result of normal, comfortable processes and treatment that ought to be chosen. This entails selection. I remember a very good aero-engine builder telling me once that he wanted every bit of every parcel of metal he used to be "the prize sweet-pea." I believe such a state of affairs can only be secured by selection.

Statistical examination of test records during the war has shown that quite a high percentage of the material thrown up by the ordinary process of production has properties markedly above the specification figures. Such material can be extracted, and if it is true, as I am often told, that a pound saved in the weight of an aircraft structure is worth a pound of gold, such material can be paid for. The normal operation of manufacturing competition will do the rest.

It is therefore in the direction of effecting improvements in manufacturing technique that action should be pressed to-day; and the term technique should be interpreted as broadly as the word improvement. In this paper it is only possible to suggest one or two lines to which attention can be directed. In making these suggestions, sight is not lost of the fact that aircraft are constantly increasing in size and that consequently the components become bigger and bigger.

Such components have either to be produced with a multitude of joints, or from much larger sub-components. The former method is more conventional but is very productive of stress raisers, and the efforts to side-step this latter trouble by welding bring fierce problems in light alloy structures exposed to high and principal stresses. Work on welding is going on constantly and at considerable pressure, but it seems that less attention is being paid to the development of large single-piece components or sub-components.

The problem has been faced for a long time in the design of engines, and a good deal of fine work has been put into the creation of complicated castings and forgings. For airframes the problem still remains—largely—but either of these forms will reduce jointing to a minimum. A third form having the same effect is large extruded sections. The last has not been neglected but the two former have not so far found any large place in aircraft structures. For them to be attractive, a good deal of development is called for with the intention of making thin forgings and castings with a large spread, in alloys possessing high strength properties. This is a technological development in regard to the castings and forgings, but a purely metallurgical one in respect of the alloys.

Certain recent work has indicated that the effect of small proportions of impurities upon the properties of aeronautical materials has received insufficient attention. This point has been rather forced on our attention by the discovery that the corrosion resistance of magnesium and its alloys is affected markedly by their iron content. The proportions of iron in question are low in any case, but a reduction of the iron content to supremely low figures has resulted in a remarkable reduction of corrodibility.

This is one example in the field of purity. Another is the great improvement in the workability of certain aluminium alloys resulting from a reduction of the impurities present. The effect may be quite widespread and it is suggested that a purification of our alloys may benefit many of their used properties. The whole subject of the effect of small proportions of non-essential elements—proportions often regarded as either unimportant or unavoidable—together with their distribution within the alloys, deserves great attention.

Modern alloys are becoming more and more complex—quaternary and even more complex systems being almost a majority. The mapping of the phase equilibrium in these systems is proceeding apace, so opening out still greater possibilities for the use of heat treatment—solution and precipitation. But,

to date, certain metals have resisted this technique, which implies fusion, and yet they seem to possess useful possibilities.

This suggests that for certain purposes advantage ought to be taken of the technique usually called "powder metallurgy." "Alloys" produced by this method can scarcely be expected to possess mechanical properties equal to those of melted and cast parts, but incapacity to fabricate by fusion should not prevent the employment of metals with helpful properties. This consideration applies particularly to metals for high temperature service, and it is reasonable that not only metallic mixtures but complexes containing metals and non-metals can thus be brought into service. For structures, articles made in this way are not yet of so much interest.

These are but a few of the ways in which the field of aeronautical metals might be cultivated. In the domain of pure research there is ample opportunity for such work as has been suggested. But work in the technological field may be almost equally rewarding and improvement of technique has by no means reached its ultimate end. Far be it from me to suggest that these two domains are to be regarded as separate and independent from each other. Each has its own special contribution to make but the best results will only emerge if the two forms of activity are synthesised.

Pure research, technological development, and productive science must go hand-in-hand, and none of the three can be neglected in favour of the other. Perhaps they may not proceed simultaneously on the same problem, but they must eventually be aligned if full rewards are to ensue. In the early days of aeronautics the deterrent to superlative manufacturing quality in metals was generally stated to be the small demand, and the consequent inadequacy of the urge and the reward. Probably that stage is past now, but a new deterrent may well appear in the form of complacency arising from the successful outcome of a war during which production requirements were met—somehow.

Pioneering is justifiable to-day as much as ever it was, though it probably costs more, and the aeronautical engineer must again call on the full energies of the metallurgist, in the laboratory and in the factory, and be prepared to support his efforts. For the metallurgist—he can remember with Chesterton that "a pioneer, men always abuse," put up with the abuse, and address himself to the task in the full knowledge that aeronautics still demand yet stronger, yet better metals, a nearer approach to perfection—and, that the engineer's demands will never be satisfied.

DISCUSSION

THE PRESIDENT. This was the first occasion, to his knowledge, on which a short summary of a Paper had not exceeded 30 minutes in length.

Before calling upon the speakers to take part in the discussion there were one or two points to which he would like to refer. The Graduates and Students' Section had drawn his attention to a tendency on the part of some members to advertise in their remarks things in which they were interested, an undesirable practice in a professional body. The Students also thought that a good deal of time was taken up in thanking the speakers. All that was necessary, except on special occasions, was a vote of thanks to the lecturer immediately after he had spoken, leaving more time for actual discussion. He now had pleasure in proposing a hearty vote of thanks to Professor Aitchison, both for his extremely able summary and his more extended Paper.

COLONEL W. C. DEVEREUX (Managing Director, Almin Ltd. Fellow). He felt it an honour to be allowed to open the discussion. He did not think there was much they could quarrel with in the statements made;

indeed there was much they could endorse. He would vigorously support Professor Aitchison in his attack on the elongation fetish. In 1930 or thereabouts he had read a Paper before the Society in which he pleaded for this sort of approach and suggested that they should have a scientific determination of the importance of this quality. In that Paper there was a diagram practically identical with that shown by the author, but not being able to put it over as well as Professor Aitchison had done he found that no one took any notice of it.

If they had explored the problem in that way there would have been available much earlier those double heat treatment materials which were really based on the theory that the elongation value could safely be reduced for certain purposes.

He would support, too, the recommendations of the author that special tests should be inaugurated to determine the suitability of materials for particular structural functions. Conventional tests had their place in research and development to indicate the probable field of new materials and to ensure uniformity, but specialised testing needed to be greatly developed.

He would not go all the way with the Lecturer in his suggestion that there should be a segregation of aeronautical materials in special classes at high prices; he thought it would be better to spend money on research to reduce costs. He believed that the impact of non-aeronautical demand for materials in peace-time did not reduce the necessary standard of excellence. A wider application by cheapening the products was just as likely to stimulate development and provide a fresh approach to many problems.

The plan suggested in the Paper for the selection of materials for aircraft would be difficult to follow out in practice. The quality of a material was so often a development of the finished form, and there were few finished forms that were interchangeable between aircraft and other uses. Perhaps sheet was one exception where it might follow, but he doubted if in practice they could work to that ideal. Neither did he think it would favour the development of the materials which they used.

He was not sure whether the lecturer in mentioning beryllium-aluminium alloy really meant definitely to encourage the audience to think that there was something just around the corner. He well remembered Dr. Rosenheim's work in the Kaiser's War, when he definitely said he could not find any possibilities in the alloying of the two materials, and that was followed by support from Dr. Desch. He had not himself seen any practical evidence that they could expect anything startling in the immediate future.

He had hoped, remembering the excellent work that the lecturer had done on the available metals in the early days between the wars and on their development—particularly in relation to high temperature steels and the working of them—that he would have been a little more adventurous in his prophecy on materials for the jet engine and turbine. If there was one way in which the metallurgist could most help the aeronautical engineer it was by continued research and development on those materials. He would like to know what hope there was of getting a material with similar properties as now, but at temperatures 200 to 300 degrees C. higher. He would like to hear, knowing the work the lecturer had done on them, what prospects there were in regard to magnesium alloys. The development in America of magnesium alloys was going along more rapidly because the material was made available to so many more users, by the lower price incentive. The wider the range for the material the stronger would be its development.

He would support whole-heartedly the development of Al-Zn structures, and could not understand why advantage had not been taken by the aeronautical engineer of the introduction during the war years of the very large stamps, presses and wider rolling mills to obtain larger structural units avoid-

ing jointing. He knew it often struck the metallurgist that the job of the aircraft constructor was making millions of holes and filling them up again providing numerous stress raisers in the process which often could be avoided by taking advantage of the plants now available to the material supplier.

He congratulated the lecturer on his excellent paper.

DR. H. SUTTON (Fellow). Professor Aitchison had given them some really provoking thoughts and there was much in the paper to engage the attention of serious-minded engineers and metallurgists concerned with aircraft materials. He thought the lecturer perhaps had been a little ungenerous to his colleagues in the metallurgical world.

He would like to touch on steels and the advances that had been made in the interval between the Great War and Hitler's War. One epoch-making advance which could not be allowed to pass unnoticed on an occasion like this was the effective use of nitriding in obtaining very high resistance to fatigue in high strength steels. To-day they had available to the aircraft engineer and designer case-hardened steels, nitrogen-hardened, the strength of which could be as high as 90 tons per sq. in. with fatigue properties far transcending those which could be obtained in the 1914-18 War in 100 ton steels. The presence of the nitrided case caused the stress raisers on the surface to be less significant owing to the initial compressive stresses there. Previously the tendency had been for fatigue cracks to start at any form of surface irregularity under extreme conditions of loading. Undoubtedly these very strong materials were very sensitive to surface conditions, and by a subterfuge of that kind they had been able to achieve a vast improvement in the resulting properties, which seemed to him to open up a possible field of advancement in the non-ferrous world. It might be that in years to come non-ferrous metals could be devised which would give similar advantages.

In the case of crankshafts and connecting rods the benefit of nitriding and the stress distribution was such as to result in an enormous increase in range of working stress. He would like to pay his tribute to those concerned in the development of case-hardening by nitriding in relation to aero-engine components.

Professor Aitchison had mentioned the rather static position with regard to some of their light metal alloys, but he had not mentioned the steady improvement in fatigue tests as observed in the laboratory on such materials as Duralumin. He could, if he had the time, produce results of laboratory tests which would show quite a steady and progressive improvement over a period of years in the fatigue properties as observed in the laboratory on materials which had not shown more than minor improvements in tensile and elongation properties. The same could be said of the light magnesium alloys, and in both cases he thought the improvement had been achieved by good technique in the casting and working of those materials, and in both types of material the improvement had been of practical value. Having had experience over a long period himself he felt he could pay tribute to the benefit of good technique in the manufacture of these products, to the advantage of the user and finally of the machine.

Another important advance, he thought, was the cladding of light alloy sheets. Under conditions of service of light alloy components small amounts of corrosion occurred, and they could have devastating effects on the fatigue properties of uncoated light alloys. He thought the cladding on the sheets was of considerable benefit to corrosion resistance and to performance in service. There were many points in the paper he could touch upon but he did not feel inclined to differ strongly with Professor Aitchison on elongation values or ductility as required by many of their specifications.

He had been studying in the past few days a rather informative paper by Brewer and

Ihsen, published in *Metal Progress* for March of this year. They had been exploring the stresses at fracture in light alloy forgings, and in their paper they gave a fairly full analysis of their results of comparable tests (static) on two different alloys, one of which had a greater ductility than the other. The tensile stresses at fracture and the breaking loads were much nearer to the values predicted by the formula for lug strength in the more ductile forgings than in the less ductile. They stressed the advantage of ductility in a product such as a forging with which they would all agree, especially those representing the light alloy industries. Ability to relieve high stresses occurring locally, by flow of metal rather than by cracking was required. He agreed with Professor Aitchison that work on high stress concentration in structures was a very fruitful field for research.

The lecturer had touched on two important points. One was the selection of material. He felt with him that if by some workable arrangement they could select the stronger batches for the special and more important aircraft applications and perhaps use for the less important applications the more normal materials available it would be all to the good. He fully agreed with the lecturer's observations on test records generally being more favourable to the material than the specification values and would like to see more advantage taken of the properties of those batches of material represented by the high-strength side of the peak on the property/frequency curves. As to the advances due to the high purity of the materials Professor Aitchison had struck an important note and he believed they would realise further progress in the improvement of their better known materials as the industry became able to supply them with purer products.

As regards beryllium there had been some interesting American work published recently on the subject of age-hardening aluminium alloys. Age-hardening characteristics had been observed at somewhat higher temperatures than they normally experienced with the age-hardening alloys with which they were more familiar. The alloys containing beryllium, although only quite small percentages were present, appeared likely to retain their hardness at raised temperatures rather better than many of the usual high strength aluminium alloys, and might have advantages for parts of engines, etc., operating at temperatures higher than atmospheric.

SIR ROY FEDDEN (Past President, Fellow). Contrary to the wishes expressed by certain of the students, as announced earlier in the evening by the President, to the effect that time should not be wasted in praise of a lecturer, he wished to refer for one moment to the work of Professor Aitchison. This was the third lecture Professor Aitchison had delivered before the Royal Aeronautical Society and he had listened to all of them. The first was in 1924, the second, he believed, was in 1934, and now this last lecture this evening in 1946. He would like to call the students' attention to each of these lectures, all of which he believed were outstanding but, without wishing to be impertinent to Professor Aitchison, he thought that the last was by far the best.

It was delightful to have an important and highly technical subject such as aircraft materials treated in the original way that they had heard this evening and he wished to congratulate Professor Aitchison on his approach to his last most finished product, and his third lecture before the Society. He believed that it was also desirable that the younger members of the Society should know that Professor Aitchison had been responsible, probably more than any other one person in this country, for obtaining that cleanliness and consistency in aircraft materials to which so much importance was now attached.

He would like to associate himself with Colonel Devereux's views about proof stress.

The sentence in the printed paper starting "A high proof stress ratio is one of the dearest wishes of the aeronautical engineer" might well go down to history and very likely engineers would, in the future, look back on it as one of the most important statements made on materials for a long time.

The engine people had perhaps learned their lesson on this question of proof stress a little earlier than the aircraft designer and had also appreciated some time ago the question of getting down to "rock bottom" on the subject of ductility. For most people concerned with engine problems this was really a matter of intense interest.

Professor Aitchison might well agree that most of the steps forward taken in engine development between the two wars had been accomplished by making up one's mind to do what at one time was thought to be impossible, and by going to the material people and shaking them up or perhaps being even rough with them and saying, "You must do the impossible on such and such a material because we are in a hole." In many such advances Professor Aitchison had taken a leading part himself.

During the period between the two wars, considerable advances had taken place, for example, on such items as chrome vanadium steel for valve springs, nickel chrome molybdenum steel for nitrided crank shafts and forged aluminium alloy for pistons. These advances had been brought about mainly because the engine constructors were in a tight corner on those particular components. Somebody had suggested that if only there could be a step up in the properties of a certain material it might be possible to get over the difficulties on the engine in question.

The engine designer had got hold of people like Professor Aitchison in collaboration with certain progressive material suppliers, who were prepared to tackle the job with a new approach, and often, a big step forward had resulted.

Never in the history of aircraft development had there been a greater opportunity than at the present time for some such similar unorthodox and bold approach, both in regard to turbine engines and also the future large aircraft.

He wondered whether as a complement to the excellent paper of this evening, Professor Aitchison could be persuaded to write a monograph in collaboration with some of their designers which would show what they could hope to do in the way of improving engine efficiency and the structure weight of their future big aircraft if certain defined steps forward in materials could be realised. For instance, if sufficient money could be spent on such developments of magnesium and high temperature steels, they might expect to take a further step forward and thus "cash in" on certain new types of machines.

If such matters could be put in the form of a straight-forward monograph it might be the best way to get the suitable Government authorities to realise that such a scheme was well worth while and would pay good dividends.

Dr. E. G. West (Technical Director, The Aluminium Development Association). He was grateful as a visitor to have an opportunity of commenting on the paper. As a mere metallurgist he cordially welcomed the paper because it crystallised thoughts which many of them had had for a long time and emphasised the need for a greater measure of intellectual collaboration with the engineer and designer in order that the most effective use might be made of the available materials. He especially welcomed Professor Aitchison's remarks on the question of how much ductility was essential in specific cases.

Dr. Sutton had forestalled him in paying tribute to the advances which metallurgists had made during the past 20 or 30 years; but he would like to sound just one warning note. He would not like members, especially the Students and Graduates, to go away with the idea that the metallurgist was looking only for new and outstanding materials

without maintaining their more routine investigations which were of considerable value to the aeronautical engineer—such as improving existing types of alloys, for example, their weldability and in particular the effect on it of "trace impurities"; or improving the wear resistance of the light alloys or possibly developing alloys of superior resistance to corrosion and erosion. All these matters were having attention.

Looking still farther ahead Professor Aitchison had mentioned briefly the advances which had been made in their knowledge of the inner structure of alloys, such as why some combinations of metals produced satisfactory alloys but others did not. This fundamental line of research based on the characteristics of the atoms of the elements would perhaps provide another approach to the ultimate goal of providing materials of superior strength/weight ratio, but it was a long-term project which required a great deal of time and money.

A further thought suggested by Professor Aitchison's paper was concerned with elastic properties. A great deal was heard about the value of a high modulus of elasticity. Undoubtedly it was often useful but it was not always necessary and there were many cases where a combination of a material of low modulus and one with a high modulus would reduce any concentration of stress which might occur at the junction of the dissimilar materials. That could be very useful when the engineer had really made up his mind what stresses a structure had to carry, and he suggested that the metallurgist could help the engineer in forwarding proposals for suitable combinations of materials in a structure.

Dr. Norman Allen (National Physical Laboratory). He would like to make three small points. The first was that in the development of a new metal or alloy the possibilities were defined very early in the course of development, but the major part of the metallurgist's effort was taken up in making those possibilities widely available in material of all forms and sizes and in getting rid of the production and other difficulties that were generally associated with the first high strength alloys which were produced. For example, the possibility of having aluminium rich alloys with a tensile strength of 40 tons per sq. in. was known in 1918. The use of such alloys was, however, not practicable in 1918 owing to their high corrodibility, and the alloys were not developed at that time. Nevertheless, continued study had in the course of time produced an alloy which, though resembling the earlier alloy in some respects, was not exactly the same as that developed in 1918, and in particular, differed in being free from many of the difficulties which had beset the earlier alloy.

The position of magnesium seemed to be rather the same now as that of aluminium in the last war. Professor Aitchison had put 23 tons per sq. in. as the present limit to the specifiable strength of magnesium alloys, but alloys were known with tensile strengths up to 30 tons per sq. in. and although it could not be said that these alloys were a practicable proposition at the moment he thought it not unreasonable optimism to hope that further study would make this higher strength available.

The second point he wished to make was rather to query the statement that new materials were not to be expected. He did not know whether Professor Aitchison really wanted to put the point as strongly as he appeared to have done. The number of metals available was very large and only a small proportion of them had come into wide use at present. Metals had come into use not in the order of their abundance but in the order of their ease of reduction. There were a great many metals with high heats of oxidation and great chemical reactivity (including, for example, titanium, zirconium and vanadium) which were reasonably abundant, but had not yet come into use because the difficulties of smelting and working them

had not yet been sufficiently overcome. The possibilities of these metals were not yet known.

His third point related to the question of elongation. It was not elongation that was wanted. What the engineer really wanted was ability in the material to deal with stress concentration. This quality was not the quality of ductility, and could be disastrously wanting in materials possessing high ductility as judged by the usual tests. Until the engineer knew what that quality was and how it could be produced, he would not know if he were on safe ground. They must find out what that property was and how it was to be obtained.

MR. C. W. SMITH (Chief Metallurgist, James Booth & Company Ltd., Associate). He thanked Professor Aitchison for his extremely stimulating address. He had been listening to a number of metallurgists some days ago in a Society meeting, and when they were asked if there had been any outstanding metallurgical discoveries during the war, the opinion was expressed that the most striking thing was the amount of what they described as "consolidation" which had taken place in metallurgy. They had learned a considerable amount about the metallurgical phenomena involved in their work during the war, and they had done something in developing it from an art to a science.

Those who spent their lives producing metals were sometimes amazed by the results which from time to time emerged. A piece of metal produced in identically the same way as another suddenly exhibited markedly better properties, and it was a strange reflection on their mentality that while they were always able to say why a metal exhibited inferior qualities, they were never able to say just why it exhibited superior qualities, and it was in that way that the lecturer had asked them to think.

To take another example of the same idea which was inherent in the lecture, it was found that if they took an aluminium alloy of the type conforming with DTD.364 in the extruded condition, they found that the extruded material would give a proof stress of 28 to 29 tons per sq. in. and a maximum stress of about 35 tons per sq. in. If they took the same piece of metal and rolled it down to sheet and treated it in exactly the same way as the extrusion was treated, they got 24 tons per sq. in. proof stress and 28 tons per sq. in. maximum stress. The extruded piece had 20 per cent. greater proof stress and greater tensile strength than the piece of sheet. The lecturer had asked them to find out why that was so.

In another connection, if they studied the question of corrosion, they found that aluminium alloy sheet consisting of equi-axed crystals was attacked very readily in a direction normal to the surface; but an extruded piece of the same alloy had a long needle-like crystalline structure with the long axes of the crystals parallel to the surface, and the corrosion penetrated at a slower rate. They ought to be thinking about these problems.

He had been very interested for some little time in the question of the effect of trace impurities. Some high purity magnesium had a remarkable resistance to corrosion, but other alleged high purity magnesium had not. This question, to his mind, was of tremendous importance in these days, and the call for consolidation was a clarion call which they ought to follow.

He could speak with less assurance on the question of elongation because it seemed to him that the materials to which Professor Aitchison had referred, namely cast iron and cast aluminium alloys, had not been subjected to extremely high stresses in the way which wrought light metals had been subjected in aircraft structures. He was told that steels as used in automobile springs should have an elongation of at least 8 per cent. if they were to function satisfactorily, but on the other hand somebody had produced a crankshaft with practically no elongation, which had given entirely satisfactory service. He would be interested to

see how engineers took up the challenge thrown out by Professor Aitchison.

MR. D. B. WINTER (Associate Fellow). He suggested that when engineers and designers realised what a material with a high proof stress and negligible elongation was going to mean to them they might recoil with horror, and that the engineer had not so much of a fetish about elongation; it was rather an instinct, and the instinct was right in this case. A casting in a material with high proof stress was likely to have some snag attached to it, and in this case Prof. Aitchison had told them that the snag was to take the form of lack of elongation. That lack of elongation was going to mean some sacrifice of workability.

He thought the designer was not going to be enthusiastic about this because if the material had a high proof stress together with low ductility he could only use that material with the greatest difficulty and in a really efficient structure to obtain its full advantage. In the case of a beam, for example, where stress through the section was in practice constantly changing it was generally only in the extreme fibres that the maximum strength of the material was utilised. If processing was going to prevent a structure of this type from being really efficient then he was going to lose the advantage of the high proof stress and would still have to contend with difficulties associated with low elongation.

There was one point with regard to ductility which he thought engineers did not fully appreciate. When it was necessary to join parts in materials of low elongation it was almost impossible to have two perfectly mating surfaces, and if the two parts had to be joined firmly one or other of the mating parts would have to undergo strain. It was possible when building up by joining a number of parts together, due to an accumulation of extreme tolerances, for considerable mal-alignment of the mating surfaces to be produced. In the case of a bolted flange, for example, tightening of the bolts might introduce bending stresses beyond the elastic limit. It was possible to calculate the permissible tolerances in such a case from the elongation of the material used to ensure that the amount of plastic bending in the flange could be accommodated without fracture.

With regard to lack of ductility, castings normally had low elongation but they usually had to be nearly twice as strong as was necessary, due to the casting factor. Lack of elongation was not a factor in the normal good behaviour of castings as castings were too massive for the lack of elongation to take effect. Incidentally, one reason why castings could not be riveted was that the elongation was small. If a fractured aircraft component had failed but not obviously through lack of elongation, it might be that it lacked the ability to absorb work. The capacity of being able to absorb work could, he thought, be taken as an additional safety factor in aircraft, in war-time aircraft particularly, where a structure might be damaged by enemy action. These points were well known to engineers but it was as well to analyse the meaning of ductility.

There should be a large amount of research into the semi-fabrication of magnesium alloys, which had not come up to expectations in that respect during the period of the war.

DR. H. ROXBEE COX (Vice-President, Fellow): He feared he could make no real contribution to the discussion but there were one or two remarks he would like to make. Colonel Devereux had referred to the work which metallurgists had done in connection with gas turbines, and he had a feeling that what they had achieved in that direction had not yet been fully appreciated. He could think of no better occasion than the present for acknowledging that the great advances in gas turbine technology over the past few years had been made possible by the progress in the improvement of materials for working at high temperatures. In the early

days of the war the metallurgists had set out to produce the right kinds of materials for the gas turbine and the work was done with a degree of determination and imagination which was worthy of the highest praise.

Professor Aitchison had made the point that engineers must always select the material which was best for the job, and must not attempt to fit the job to the material, and he would like to know whether the Professor saw any prospect of taking the further step forward of *designing* materials for the job. In a manner of speaking that was what had been done in the case of gas turbines, and he wondered whether there were any hopes of it approximating to a precise science. It was evident that they wanted materials to do a lot of very different things. In the gas turbine field they wanted materials to work at extremely high temperatures, much higher than at present. Also, they would like materials with very high damping capacity. Professor Aitchison had referred to the stiffness of aeroplane wings, and in that connection it would be an advantage to have a material with a very much higher Youngs Modulus than steel. Could such properties be "designed into" materials?

Professor Aitchison's contributions were not entirely in the field of metallurgy. He had made a contribution in this paper to nomenclature which was noteworthy. He had referred to the years between the wars as "the isthmus years"—a very neat expression which would be very useful in the future.

Mr. R. H. CHAPLIN, O.B.E. (Hawker Aircraft, Ltd., Fellow): Professor Aitchison had made the provoking suggestion that use should be made of materials having little or no elongation. Frankly, he was horrified at the suggestion; he thought that a reasonable degree of elongation was absolutely necessary for various reasons.

One such reason was the presence of strain which was unintentionally built into aircraft structures during manufacture. This was particularly noticeable in light alloy castings which typically had low elongation values. He was not so fortunate as the lecturer in not having had any failures. He had had innumerable failures both in assembly and service. Mr. Winter had referred to the impossibility of riveting up castings without fracture to some other part of the aircraft structure and this was typical of the difficulties encountered. A recent experience of such fracture was in the sleeve portion of a casting which slipped on a tube and was located by a bolt. In these particular cases a distance tube had not been included and the small deformation due to tightening the bolt had caused fracture which would not have occurred if the material had possessed a reasonable elongation.

The lecturer had referred to the absence of failures of castings in service, but, as Mr. Winter had pointed out, this was explained by the fact that they were twice as strong as they needed to be. Experience had shown this safety factor of two was necessary because of the unreliability of castings.

The lecturer, or one of the earlier speakers, had suggested that stretch was not allowable in aircraft structures. This was wrong. It was a feature of aircraft design that a structure was allowed to stretch at loads above 75 per cent. of the fully factored load.

Elongation was undoubtedly of value when suddenly applied loads of high magnitude were being considered. With such a load the amount of energy behind it was important and it was the elongation of the material which provided the facility for absorbing that energy and thus avoiding failure of the structure in such an emergency.

Reverting for a moment to light alloy castings, he would like to ask metallurgists and the metal industry why one had not had the satisfactory service to which Professor Aitchison had referred. A very rigid inspection was officially insisted upon to ensure

that castings were sound. This not only caused a high percentage of scrap in production but also delayed the initial castings so much that it was not wise to include them in a prototype design. Were the methods of inspection either unreal or too severe, or were castings really so unpredictable?

One speaker doubted the necessity or the desirability of high modulus of elasticity in aircraft materials. A high modulus was necessary for two reasons; firstly, modern aircraft were designed as much to satisfy stiffness requirements as they were to satisfy strength requirements; and secondly, to obtain the stability necessary in developing the highest possible stress in the material when under compression in a semi-stable state such as existed in the panels and stringers of a monocoque, or the flanges of a rolled strip section in bending.

Although the lecture had emphasised developments in the light alloy field any reference to development in the ferrous field should include the tremendous advance which had been made in steel castings. These castings, which now possessed high ultimate strength and high values of elongation, had given his firm very satisfactory and consistent service.

Mr. N. S. Muir (Associate Fellow): Not being a metallurgist he could not contribute directly to the discussion, but he would like to support strongly what had been said about the possible degree of improvement that could be achieved in jet engine materials, and also to support the suggestion made by Dr. Roxbee Cox that new materials might yet be designed.

Another point he would like to mention was the matter of plastics. In the engine field it seemed there was considerable scope for the use of plastics, and it occurred to him that in the very important component of the jet engine, such as the centrifugal supercharger, if they could get a suitable plastic possessing a ratio of specific gravity to proof stress comparable with those of the alloys used at the moment it might be a very successful substance to use, since the plastic impellor could be formed to exactly the same dimensions as the original. A small investigation he had made indicated that some present day plastics came very close to the requirements.

Another important advantage was that plastic material might well supply a considerable measure of internal damping against vibration which Dr. Roxbee Cox had indicated was needed. Could Professor Aitchison say whether in such plastics there would be any use in introducing some metallurgical bond in the form of metallic powder to get heat flow through the material, since the plastic materials of to-day had rather a bad heat capacity and transfer property?

Mr. N. Nicholas (Fellow): A long process of annealing was required when making castings of magnesium-aluminium alloys. Was there any method of shortening the annealing period, or dispensing with annealing altogether?

Dr. W. D. Douglas (Royal Aircraft Establishment, Fellow): Metallurgists had not had a square deal from the aeronautical industry. The metallurgist was responsible for producing materials for the industry to use and it was not for the metallurgist to tell the industry everything that the materials could do, but rather for the aeronautical engineer to tell the metallurgist what the materials should do. There was a state of ignorance among them as to what some of the interesting new materials would do, and how best to use their properties. A lot of work was being done to clear up this defect in knowledge.

In designing a part, there were three main steps. One was to estimate the type and magnitude of the stresses which that part would have to withstand; another was to estimate what physical or mechanical properties of the material would contribute towards sustaining the stresses; and the

third was the calculation of how much of the material was needed in a particular place. The first and third of those factors were the concern of the aircraft designer or aeronautical engineer.

First of all, the designer did not know accurately what stresses the part was going to experience in service; he could only make an approximate guess, which would have to be expressed in statistical terms. The same applied to the calculation of how much of a given material would be wanted.

If they took a simple property like tensile strength, they frequently knew only the average strength of a test piece, or rather the average load it could withstand before it broke. Very few people could foretell the maximum stress which a material could withstand without breaking; a sample of the material had merely been put into a testing machine and loaded until it broke. What load could safely be applied and removed without the material breaking? They might claim that survival and fracture stresses were near enough the same but was there any experimental verification of such a statement? In general, if they were interested in tensile strength they should have some idea of the load which a test piece could survive without breaking.

But that was not the whole story. Professor Aitchison had pointed out that the change in average tensile strength indicated the improvement that had occurred during the isthmus years. Improvement was evident but it would be more evident if they could have information on the change of coefficient of variation in these materials. If the criterion of strength was the average strength, less some number of times the coefficient of variation (or standard deviation), then a decrease in average strength might be more than offset by a reduction in amount of variation.

It was important that they should go on doing a large amount of work on finding out what properties they wanted in their materials and how to express in suitable form the properties of the materials which existed. That was a necessary first step before they could utilise efficiently their new materials or produce materials designed for a particular job.

Mr. C. GURNEY (Royal Aircraft Establishment, Student): If it had not already occurred, failure must occur when a metal melted.

The melting temperature normally quoted for materials was the temperature at which melting occurred in the absence of stress other than the very small stresses caused by atmospheric pressure. When a material was stressed, the melting temperature changed, and it was possible by thermodynamic reasoning to calculate the gradient of melting temperature with respect to any of the applied stresses. Natural processes tended to a reduction in free energy of thermodynamic systems. The simple stress system which resulted in the biggest temperature gradient of stress was unidirectional compression because in that case, if the liquid flowed out at right angles to the direction of compression, the external forces did work given by $P.V.$ where P was the compressive stress, V the volume of the solid which had melted. The liquid, being stress free, immediately froze. For this case the formula for the stress coefficient of melting, neglecting some unimportant terms was

$$\frac{dT}{dX} = \frac{TV}{L}$$

where T X V and L were melting temperature °K, stress ($+ve$ tensile), specific volume, and latent heat of melting respectively. Over a moderate range of stress, V and L could be assumed independent of stress and temperature. The expression could then be integrated, and the stress at which the melting temperature was equal to the room temperature estimated. Unless the metal was capable of being superheated, and there was at present no experimental evidence for superheating of metals, the material must fail

at this stress. The formula for the maximum stress was (tension $+ve$)

$$X = \frac{L}{\Gamma} \ln \frac{T \text{ room}}{T \text{ melting stress free}}$$

Using this formula, the maximum compressive strength of a number of elements had been calculated and was given in the following table.

Metal	Al	Be	Cu	Fe	Pb	Mg	W
Stress Tons in^2	65	220	150	205	11	30	470

These figures would not normally be improved upon by alloying, the melting point of one of the constituents of an alloy being usually lower than that of the constituent, pure metal having the highest melting point.

Greater ductility would be necessary in materials for military than for civil aircraft. Military aircraft had to withstand combat damage such as was caused by bullets and shell splinters and such damage caused high concentration of stress.

Ductility or brittleness were not just properties of a material. They also depended on the stress system, stress gradient, rate of increase of load and temperature. In general, stress systems involving high compressive stresses, low stress gradients, low rates of loading and high temperatures were conducive to ductility and their opposites were conducive to brittleness. A material with very small elongation on a tensile test might flow under the bearing pressure of a bolt sufficiently to cause the required stress relief.

Mr. P. F. Crawley (Student) *contributed*: In the discussion following Professor Aitchison's paper, no engineer attempted to answer the vexed question: "How much elongation do you want," He was no more qualified to give an answer than other people, but the appraisal of the situation given by the metallurgists might be assisted by the engineer's point of view. Of necessity, they could not consider the subject of "Materials" apart from "Structures" in aeronautics and that was the major trouble. Not until the engineer knew more about his structures could he give an order for made-to-measure materials.

The ductility required by engineers, as pointed out by Dr. Norman Allen, was not in itself important — it was the measure it gave of the relief at stress concentrations possible with a given metal. Naturally engineers would wish to learn better how to control these stress-raisers, but they felt that where the metallurgist could give considerable assistance was in the concentrated practical and theoretical study of much stress-concentrations. They wanted to know in what way the ductility was in fact an index of the efficiency of stress distribution round holes.

The engineer was in a position to hand back the question to the metallurgist, since if the latter could show him that metals with low elongation values had in fact as high a notch strength as those in common use to-day, he would be only too glad to use them.

In the structural field, far better use of the material available could be made than was at present the case, and Professor Aitchison's suggestions were extremely helpful. For instance, a great saving could be made if they could design structures so that members could be in tension or compression only under all loading cases, as they could then take advantage of the specialised materials advocated.

Mr. Russell's paper on thickness of light alloy sheets showed another field where technique was at fault, and, as suggested in the discussion, statistical work on metal strength would yield good dividends.

As technique and stressing methods improved the factor of ignorance might be lowered and it was felt that here was the greatest single factor which would contribute to the more efficient use of materials.

He had two questions: Firstly, how did a metallurgist set about discovering a new alloy of special properties? Had he generalised figures on effects of heat-treatment

techniques and effects of particular alloying constituents, or had he still to tackle it in a hit and miss method more reminiscent of engineering practice?

Secondly, some interesting figures on crushing melting stresses had been quoted by a speaker. He understood that these were calculated figures and it would be interesting to know whether they had been experimentally confirmed.

R. H. SANDIFER (Handley Page Ltd., Fellow) *contributed*: He did not think that enough research had been done to enable the engineer to state exactly how much or how little elongation he required, but it was worth noting that although he had successfully employed castings with very small elongation, those same castings had not been highly stressed, compared with wrought materials, including the light alloys. Therefore the strains induced had also been small and corresponding differences of strain between one part of a member and another, in the vicinity of bolt holes, also not large. The requisite amount of elongation therefore appeared to be directly related to the maximum design stress, and to increase with it. This was particularly necessary in the design of multi-bolted joints, and, subject to further experimental evidence he would suggest that they had already reached the lower limit of elongation for 35 tons/in^2 light alloys, at 6 per cent. to 8 per cent.

Another aspect of the same problem arose from the specified design requirements, to which they worked, *viz.*, a proof factor of 1.125 and an ultimate factor of 1.5 on the maximum expected loads. This gave a ratio of proof to ultimate of 0.75 and therefore any increase of proof stress in a material beyond 0.75 of the ultimate stress was of little advantage, especially if this was at the expense of elongation.

Professor Aitchison's remarks on the hope of increasing the value of Young's Modulus for light alloys were encouraging, and it appeared that further research work on aluminium-beryllium would well repay the effort; in fact, from the figures given in table 2 one would expect the optimum solution would come from about 32 per cent. beryllium. There was a real need for a light alloy with an improved value of "E." Wing deflections, both flexural and torsional, were already presenting serious problems to designers.

Finally, with regard to improvement of consistency in quality, one way in which the disadvantages of a wholesale up-grading of specifications could be avoided, would be to admit of grades "A" and "B" within the same basic specification, so that a minimum amount of material produced would be wasted, while at the same time the best would be utilised profitably.

Mr. G. L. BAILEY, M.Sc. (Director of the British Non-Ferrous Metals Research Association) *contributed*: He was extremely sorry that he had been prevented from attending the lecture. Professor Aitchison had set out the position from the metallurgical point of view extremely well. The section he was most interested in was that which concerned the future requirements of the aircraft industry. The metallurgist was constantly striving to develop better materials, and looked largely to the engineer for guidance as to the directions in which improvement was most urgently needed. It was in the hope of receiving such guidance that he would have liked to have attended the meeting.

In the past emphasis had been mainly laid on improvement in proof stress and a great deal of success had been achieved in this direction, but the important question of the extent to which elongation could be sacrificed in this quest for a higher proof stress was one which he was at least as anxious as Prof. Aitchison to see answered. He would like to take this question a little farther and ask the engineer whether he was really only interested in total elongation as normally measured on a test bar, or whether he was

interested to see that elongation divided into its component parts — "general" and "local." Once local elongation took place there was an increased local reduction of area which would lead to a considerable increase in stress if the load remained the same. If elongation was required only to allow relief of local high stresses by throwing the load on to some other part of the structure, this might not matter, but he would like to see any analysis of the engineer's requirements in the matter of elongation take into consideration the importance, if any, of uniform general elongation. Elongation was low in all these materials. If it was general elongation that mattered it would be important to know just what amount was available without having to draw on that doubtful reserve of local elongation which was of no real value to the aeronautical engineer however much it might interest the metallurgist for other reasons.

A second point which had not been fully dealt with by Professor Aitchison was the importance of fatigue limit. Professor Aitchison discussed the possible importance of the imposition of intermittent fluctuating stresses of high magnitude on either steady or fluctuating stress cycles of relatively low magnitude. He shared his interest in that point, but was not satisfied that enough attention had been paid to the importance of fatigue as normally understood, *i.e.*, a steady fluctuating stress cycle of medium magnitude.

The information available on the fatigue resistance of aluminium alloys generally used even under these simple conditions was not as complete as he would like to see it, but how important was it? As far as he could tell the improvements in proof stress of aluminium alloys had not been found to be accompanied by corresponding increases in resistance to fatigue. Admittedly there was in many cases some improvement in endurance limit as proof stress was raised, but such improvement was rarely proportional to the improvement in proof stress. If, therefore, the engineer designed primarily on proof stress, was he in danger of running into fatigue failures, or alternatively was he unable to take full advantage of the higher proof stress because there was no corresponding improvement in fatigue resistance?

That brought them back to the question which was uppermost in his mind, namely, what were the limiting factors as regards mechanical properties which governed the use of materials in aircraft structures? It was no use concentrating on proof stress if there was a limitation imposed by elongation, fatigue resistance or any other property, and the metallurgist would be tremendously helped in his quest for better materials for aircraft construction if the aeronautical engineer could define his requirements more closely.

One other point Professor Aitchison had not touched upon, but which should be borne in mind, was the presence and effect of internal stresses. Certain types of heat treatment designed to produce improved mechanical properties in the test bar might well introduce undesirably high internal stresses, the presence of which greatly offset the advantages of the high proof stress. This was becoming increasingly appreciated, but was worth recording again particularly where the conditions of service were such as to render the material liable to stress corrosion. It might, in fact, be desirable in some instances to use material of lower mechanical properties if those properties could be obtained substantially free from internal stresses which might result from forcing the material to give high test bar values.

THE PRESIDENT: The hour was late and they had had an interesting discussion. The question of elongation was most interesting. If the structure were not resilient it would transmit more readily any high-frequency vibrations which might well arise. The effect would be like riding in a stiffly

built ship, which would produce seasickness in the passengers. He was not so certain as one of the speakers about ductility not being so necessary in civil aircraft as in military aircraft. He would now ask Professor Aitchison to reply.

PROFESSOR AITCHISON'S REPLY

He appreciated the really interesting discussion and felt his paper was justified simply because it had produced such a discussion. It had been his intention to be provocative, and he was sorry he had not provoked the meeting even further. Nevertheless they had not answered his question as to how much elongation was really wanted. He was going to leave the question with them and not deal with it any more. It was necessary to find an answer in some form or another so that engineers and metallurgists could know where they were heading. Neither could balance their metals until they really knew what properties were required of them. So an answer to the question was really necessary.

Several very nice things had been said about him and about his influence on metallurgical development, and for these he was grateful. He thought he must answer most of the discussion in writing.

He would, however, like to point out to Dr. Allen that throughout the paper he had tried to make it clear he was quoting specification figures. The value of 23 tons for the magnesium was the best he had met in these papers, and he did not think any specifications yet had ventured on a 30 ton figure. He felt too that there had been some misapprehension in what was intended in the paper as to the lack of development on the strength of steel. What he said was that the alloys, *qua* alloys, gave 100 tons in 1918, and the value was only 100, *qua* alloy, now. One might, and some did, find ways of combating the difficulties, and of preventing the incidence of failure by such methods as nitriding, but the basic material itself was not thereby made intrinsically stronger. He wanted to make that point now because he felt there might be a genuine misunderstanding about the statements made in the paper.

In conclusion he thanked all who had taken part in the discussion.

Written:—He would like at the outset to repeat and emphasise one or two things he had said hastily in his brief verbal reply. The first was to thank, more heartily than he could then, all who took part in the discussion and who dealt so kindly and tolerantly with the views he put forward, provocative though they were, and who in addition made such complimentary remarks about his metallurgical work. His gratitude was also sincere to the several who, like Dr. Douglas, Dr. Allen and Mr. Bailey, enlarged on certain of his views and gave them fresh point and greater clarity.

The second was to emphasise his regret that anything he said could be construed either as critical of, or unjust to, those who had toiled in the metallurgical field during the past thirty odd years. When he read the paper he was a little fearful that such a misapprehension might arise, and perhaps he ought to have gone farther out of his way to remove any possibility of it in anticipation. He must however repeat that it really was a misapprehension, because although nothing came forward in the discussion to dispute the broad view that the actual strength values of the available aeronautical metals of construction *per se* were not much higher in 1944 than in 1918, he recognised, and hoped to convey the view, that it was by reason of the labours of the metallurgists during those years that the engineer was able to use a much higher proportion of these intrinsic strengths and to rely on his materials to a far greater extent than was previously

possible. For this achievement every possible credit went to the metallurgists, including those who had worked out such various processes as those mentioned by Dr. Sutton, designed to combat metallurgical inferiorities and sources of weakness, and to enable fuller advantage to be taken of the latent capacities of aeronautical metals.

Thirdly he must again enlarge on the point that his paper was written on the avowed basis of the standard specifications (either B.S. or D.T.D.) for aeronautical metals, and the values taken for the mechanical properties of the different groups of material were those embodied in those specifications, whether actually issued, or in draft form. He had also, and he thought justifiably, excluded those examples where the specified strength of the metal was due to any notable application of cold work, his real concern being with the intrinsic properties of the metal arising from its composition, constitution, thermal treatment and the like.

Reviewing the discussion as a whole, and it seemed to lend itself to such a treatment rather than that of replying to a series of individual remarks or queries, one might at first say the meeting divided itself into two sections, one rather on the offensive and the other ranged in defence. A deeper examination of the contributions revealed a greater unity than was at first evident.

In the first place, nobody was concerned to deny the apparent halting of the advance on traditional lines towards greater strength, or to make any particular suggestion as to how further improvements should be achieved—if at all. Dr. Allen's point that there were other metals, beyond beryllium, deserving of examination and possible development, was well made and he agreed wholeheartedly.

He was equally interested in Mr. Gurney's most valuable work directed to assessing the maximum theoretical strength of a metal, and thanked him for the values he had quoted. (Was he right in presuming that one exception to the normal effect of alloying might arise in a system, such as the magnesium-tin, in which a phase existed with a higher melting point than that of the constituent elements?) Mr. Gurney's figures were of the utmost interest when taken in relation to the achieved strengths of metals and certainly did not run counter to his suggestion that the known industrial metals were approaching their optimum properties.

A second point of real unity appeared to be the generally held opinion that they did not know sufficient about the essentially requisite properties of aeronautical materials. If he had needed any further convincing, the discussion impressed on him more firmly than ever the need for looking at the properties of metals as a whole, and for examining those properties not as isolated values but as the basis of components required to serve and function in particular environments and under different conditions and sequences of stress.

It was quite evident that the meeting was unable, rather than unwilling, to answer such a question as that posed on the value of elongation which an engineering metal must necessarily possess. This was not surprising, and he believed that the greatest surprise would have been the emergence of an immediate and precise answer. The manner in which the meeting seemed to divide was to separate those who were horrified at the bare suggestion of reducing the traditional elongation values, from those who agreed it was important to find out how much the value really meant, and in what way and to what extent its meaning was affected by conditions and environment. Naturally he was on the side of those who would like further investigation, and he was grateful to those many contributors who took up his point, amplified it, and put it into a better framework.

There seemed to be a reasonable agreement that the time-honoured elongation value was not very revealing and that the

property sought in materials of construction was a capacity to flow sufficiently to accommodate the effects of stress concentration, and to do so under the circumstances dictated by the different conditions of service. This capacity might be a kind of ductility but might not be the same kind that resulted in a high value of elongation being recorded in a tensile test. Neither need the possession of the desired kind of ductility be at all inconsistent with the desire for a high proof-stress ratio or a higher modulus of elasticity. Despite the adherents of high elongation, he felt that the meeting made out a good case for intense research in this field and he only hoped such a programme of work could be instituted and prosecuted.

Although on this particular issue he had shown on which side his personal desires lay, he was not at all sorry that the other view was expressed, and particularly so because it seemed to be centred round two things. The first was certainly a fear of the consequences of abandoning the existing elongation criterion, and such a fear emphasised the need for research and investigations, rather than otherwise. The second focus was the experience of castings, a form of relatively low elongation material mentioned in the paper.

During the discussion he could not help feeling that the difficulties with castings might have been more fully analysed and the causes of trouble more precisely set out. He got the impression that although castings had given trouble, this was not necessarily because they were deficient in elongation, or even because cast metal had an intrinsically low ductility, but more probably because the castings were uneven in quality and some of them were defective in a metallurgical sense. If this reading of the situation were correct, these contributions to the discussion served to strengthen the view given in the paper that the second principal field for work to-day was that of technological improvement and development.

He had no doubt of the need for more basic research on the properties of metals, and the meeting undoubtedly was with him. But he reiterated his equally strong conviction that they were far from perfect in their productive methods. On this point the meeting was relatively silent, except by inference, and he thought it needed emphasis. To make a perfect casting, for instance, was a very difficult thing, and to make a thousand perfect castings, all in a row, was still more difficult. But the reward of success was likely to be very high, both to the user and to the successful manufacturer. The same applied to other metallurgical articles, but since so much was said of castings they might serve to point the moral.

He thought that he had really covered the essence of the discussion and the correspondence, and would prefer to leave it so. He was intensely grateful to all who contributed, and filled out the views he could only express generally. He hoped their generous support would result in focussing attention on those points needing further elucidation and development, so that there might be a still further advance in aeronautical metals.

THE ROYAL AERONAUTICAL SOCIETY

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STRENGTH OF SCREW THREADS OF WHITWORTH FORM

by

J. V. INGLESBY, M.A., D.I.C., Assoc. Fellow

Mr. Inglesby read Mechanical Sciences at Cambridge, obtaining his degree in 1935. He has had experience in the Stress Office, Shops, and Propeller Stress Office with Saunders-Roe, Ltd., Vickers-Armstrongs, Ltd., and the de Havilland Aircraft Co., Ltd., respectively. After some three years working on propeller flutter and other problems with the Ministry of Aircraft Production, and later Ministry of Supply, he is now working at the Guided Projectile Establishment.

Summary

A STRESS analysis of screw threads of Whitworth form has been performed.

The critical stress in the threads is a shear stress, and this may be expressed simply in the form:—

$$s_{max} = f.\frac{kP}{LD}$$

where

s_{max} = shear stress.

f = distribution factor.

= 1.0 when screw and nut are both in tension or compression.

= 2.0 when screw and nut are in tension and compression respectively, unless specially designed for an improved distribution of load.

k = coefficient, depending mainly on fit, but also on pitch of threads, L and D.

<0.8 for close fit.

<0.9 for medium fit.

<1.0 for free fit.

P = axial load.

L = length of engagement.

D = nominal diameter.

1. INTRODUCTORY

Frequently, especially in aircraft design, it is necessary to reduce the size of certain parts, as rivets and bolts, to a minimum in order to facilitate fitting or assembly. Moreover, the importance of saving superfluous weight in aircraft design can hardly be overemphasised. When the strength of a component is known only vaguely, oversize is inevitable, to allow for the "factor of ignorance." Such considerations prompted this work.

To date there is no recognised method of stressing Whitworth form screw threads given in any text book on engineering or engineering handbook.

2. WHITWORTH FORM

Screw threads of Whitworth form have been adopted as standard engineering practice in Great Britain and throughout the Empire.

The thread is a symmetrical V-thread in which the angle between the flanks is 55°; the crests and roots being rounded equally

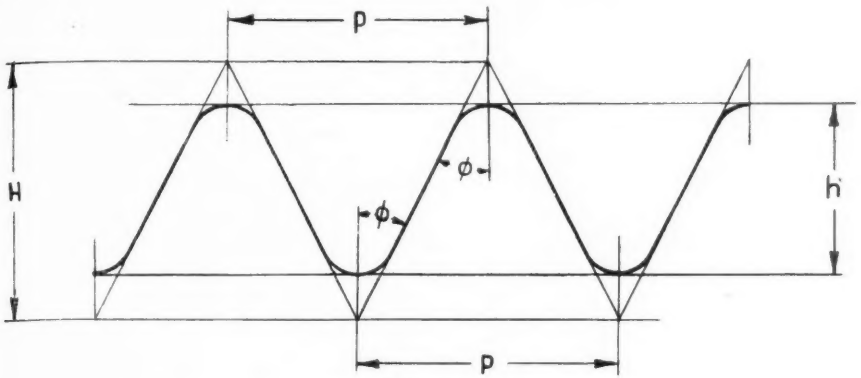

Fig. 1.
$H = \frac{8}{5} h$
$\phi = 27.5°$

by circular arcs blending tangentially with the flanks. See Fig. I.

Full details of the thread are contained in B.S.84-1940, " British Standard Specification for Screw Threads of Whitworth Form," Ref. 1.

3. ASSUMPTIONS

Before proceeding, certain general assumptions must be made. These will be classified under two headings:—

a. Thread.

b. Pressure.

a. Thread.

i. Screw and nut to be perfectly axial.

ii. Thread profile to conform to the British Standard Whitworth form, with correct flank angles and arcs between the flanks.

iii. The pitch of the threads to be correct.

iv. Effective diameters to be on worst limits; low limit for screw and high limit for nut.

v. Helix angle small.

vi. Threads on a rigid base.

b. Pressure.

i. Distribution.

The distribution of pressure between engaging threads to be uniform, both along and across, the surfaces in contact.

(Later, actual load distributions along the length of engagement will be considered, but in the first instance, uniform distribution will be assumed.)

ii. Direction.

The pressure between engaging threads to act in a direction normal to the surfaces in contact.

If the load is taken up by screwing, there will also be a frictional force between the surfaces, along the thread-helix, opposing the direction of rotation. However, this frictional force does not affect the strength of the threads and will be neglected.

These assumptions should be a reasonable basis for a stress analysis. The conditions are pessimistic, but quite possible, without being unnecessarily cumbersome.

4. REVIEW OF STRESSES

To obtain a better conception of a pair of loaded threads, let the threads be uncoiled, so that now two long straight threads bear on each other. In this transformation the

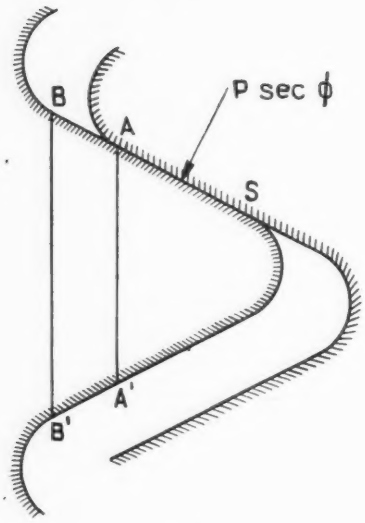

Fig. 2.
P = axial load
ϕ = flank angle
P sec ϕ = bearing load

internal stresses are unchanged, except for the effect of radial restraint. However, this effect is small if the depth of thread is small compared with the diameter. So then, the threads act as beams, though of somewhat unconventional proportions. On any axial section there will be a shear stress due to the shear force, a bending stress due to the bending moment and a compressive stress due to the oblique angle of the applied load. Also there will be a bearing stress on the surfaces in contact. See Fig. II.

For convenience, the applied pressure will be resolved into components:—

a. Axial.

b. Radial.

By inspection, it is seen that the following stresses are of importance:—

i. On surface SA.

Bearing stress due to pressure between the surfaces. This is simply the normal pressure.

ii. Section AA′, at neutral axis.

Section AA′ is through the thread axially, at the line where adjacent surfaces separate. The maximum shear stress due to the axial component occurs at the neutral axis and this combines with a compressive stress due to the radial component. There is no flexural stress here.

iii. Section BB′, outer fibre.

Section BB′ is through the thread axially, where the root arcs blend with the flanks. The maximum flexural stresses due to offset loading occur at the outer fibres, and these combine with a compressive stress due to the radial component, so that the total compressive stress at B′ is critical. This stress is affected by stress concentration at the root arc. There is no shear stress here.

To determine the relative importance of these stresses, a particular case is worked out in the Appendix.

5. CRITICAL STRESS

From the case worked out in the Appendix it is clear that the critical stress in the thread is the maximum shear stress at the neutral axis of section AA′, and this is unaffected by radial restraint. Observations of the failure of screw threads confirm this conclusion.

The maximum shear stress may be expressed:—

$$s_{max} = f.\frac{kP}{LD}$$

s_{max} = max. shear stress (tons per sq. in.).
P = axial load (tons).
L = length of engagement (inches), minimum on tolerances.
D = nominal (major) diameter (inches).
k = coefficient. This depends mainly on the fit—close, medium or free—but also on number of threads per inch, length of engagement and nominal diameter. The value of k will differ for external and

internal threads. Values of k are given in Tables I and II.

f = distribution factor.

This is introduced to allow for the distribution of load along the length of engagement:—

a. Tie.—The nut component and screw component form a tie. Both components in tension. Example: Rigging screw.

b. Strut.—Both components in compression.

c. Clamp.—The nut component and screw component form a clamp. The nut component is in compression while the screw component is in tension.

For tie and strut the load may be assumed to be distributed uniformly along the engaged length of thread. Hence $f=1$.

TABLE I

VALUES OF k

EXTERNAL THREADS

Threads per inch	Length of engag. dia.	Nominal dia. 1/2 in.	1 in.	2 in.	4 in.	8 in.	Fit
	1	0.856	0.891	0.980			
32	1/2	0.829	0.848	0.906			
	1/4	0.810	0.819	0.860			
	1	0.828	0.794	0.803	0.844		
16	1/2	0.817	0.778	0.778	0.804		
	1/4	0.809	0.766	0.761	0.778		
	1		0.800	0.758	0.753	0.772	Medium
8	1/2		0.793	0.747	0.737	0.748	
	1/4		0.788	0.739	0.726	0.732	
	1			0.782	0.735	0.723	
4	1/2			0.777	0.727	0.712	
	1/4			0.774	0.722	0.705	
	1	0.792	0.800	0.839			
32	1/2	0.776	0.776	0.802			
	1/4	0.765	0.760	0.777			
	1	0.799	0.756	0.751	0.769		
16	1/2	0.792	0.746	0.736	0.746		
	1/4	0.787	0.739	0.726	0.731		
	1		0.781	0.733	0.721	0.728	Close
8	1/2		0.777	0.727	0.711	0.713	
	1/4		0.773	0.722	0.704	0.703	
	1			0.770	0.719	0.702	
4	1/2			0.767	0.714	0.695	
	1/4			0.764	0.711	0.691	
	1	0.975	1.077	1.311			
32	1/2	0.923	0.985	1.125			
	1/4	0.889	0.929	1.023			
	1	0.877	0.860	0.898	0.991		
16	1/2	0.858	0.831	0.851	0.911		
	1/4	0.845	0.812	0.821	0.862		
	1		0.830	0.797	0.807	0.851	Free
8	1/2		0.818	0.779	0.780	0.808	
	1/4		0.810	0.767	0.762	0.780	
	1			0.802	0.760	0.756	
4	1/2			0.794	0.748	0.739	
	1/4			0.788	0.740	0.727	

For the clamp case it has been found, Ref. 2, that there is a concentration of load distribution towards the bearing surface of the nut, which is approximately twice the mean. Whence $f=2$.

Special nuts have been designed to relieve this concentration. In such cases, $f<2>1$. The exact value depends on the particular design. Until further data is available, guidance can be obtained from Figs. 13-19, Ref. 2. For most types of special nut, f varies between 1.5 and 1.2.

Tables I and II contain values of k for external and internal threads respectively. If the engaging threads are of the same material, values of k from Table I must be used. If the engaging threads are of different materials, the threads of the weaker material are critical, and the table chosen accordingly.

The two threads of a mating pair are

TABLE II

VALUES OF k

INTERNAL THREADS

Threads per inch	Length of engag. dia.	Nominal dia. 1/2 in.	1 in.	2 in.	4 in.	8 in.	Fit
	1	0.818	0.874	0.972			
32	1/2	0.790	0.830	0.897			
	1/4	0.771	0.801	0.851			
	1	0.740	0.755	0.785	0.836		
16	1/2	0.728	0.737	0.759	0.795		
	1/4	0.720	0.726	0.742	0.769		Medium
	1		0.710	0.717	0.734	0.763	
8	1/2		0.702	0.706	0.718	0.739	
	1/4		0.697	0.698	0.707	0.723	
	1			0.691	0.693	0.703	
4	1/2			0.686	0.686	0.692	
	1/4			0.682	0.681	0.685	
	1	0.752	0.781	0.830			
32	1/2	0.736	0.757	0.793			
	1/4	0.725	0.741	0.768			
	1	0.709	0.715	0.731	0.760		
16	1/2	0.701	0.705	0.716	0.737		
	1/4	0.696	0.697	0.706	0.722		Close
	1		0.690	0.692	0.701	0.718	
8	1/2		0.685	0.685	0.691	0.704	
	1/4		0.681	0.680	0.684	0.694	
	1			0.678	0.677	0.682	
4	1/2			0.674	0.672	0.675	
	1/4			0.672	0.669	0.671	
	1	0.942	1.062	1.305			
32	1/2	0.887	0.969	1.119			
	1/4	0.853	0.912	1.016			
	1	0.793	0.823	0.881	0.983		
16	1/2	0.772	0.793	0.833	0.903		
	1/4	0.758	0.773	0.803	0.853		Free
	1		0.743	0.758	0.789	0.842	
8	1/2		0.730	0.739	0.761	0.799	
	1/4		0.721	0.727	0.743	0.771	
	1			0.712	0.719	0.737	
4	1/2			0.703	0.707	0.720	
	1/4			0.698	0.699	0.708	

assumed to be made to the same grade of fit. When different grades are specified, an average value of k for the two fits is recommended irrespective of which thread is critical.

Values of k in the tables have been calculated for the basic shape of thread:—

$$H = 0.960491 \times p$$
$$h = 0.640327 \times p$$
$$r = 0.137329 \times p$$
$$\phi = 27.5°$$

where H = depth of fundamental triangle.
h = depth of thread.
p = pitch of thread.
r = radius of crest and root arcs.
ϕ = flank angle.

For medium fit, the effective diameter tolerance used is expressed:—

$$t = 0.002\sqrt[3]{D} + 0.003\sqrt{L} + 0.005\sqrt{p}$$

where t = tolerance.
D = nominal (major) diameter.
L = length of engagement.
Units of length in inches.
For close fit, the tolerance is $\frac{2}{3}t$.
For free fit, $\frac{3}{2}t$.

6. WORKED EXAMPLES

a. ¼in. BSF duralumin nut.

To calculate max. safe load.

Material — Duralumin L1
Specified shear stress = 16 tons/sq. in.
Clamp $f = 2$
Threads per inch = 26 (medium fit)
Nominal dia. $D = 0.25$
Nut thickness = 0.190—0.200
$\therefore L = 0.190$
$\therefore L/D = 0.76$
$\therefore k = 0.755$
Table II (extrapolation)

$$\therefore \text{Safe load} = \frac{s_{max}\, LD}{fk}$$
$$= 0.5 \text{ ton}$$

A safety factor of 2, 3 or 4 is generally required. This is to allow for possible freak loads, faulty manufacture or imperfect assumptions in formulating the method of stress.

b. Propeller hub retaining nut.

To calculate reserve factor.

Material		
Propeller shaft	Steel S11	
Hub retaining nut	Steel S11	Aluminium bronze DTD197
Propeller thrust	= 6,500 lb.	
Safety factor	= 4	
	$\therefore P$ = 26,000 lb.	
Clamp	$f = 2$	
Threads per inch	= 12 (medium fit)	
Nominal dia.	$D = 3.625$	
Length of engagement	= 1.25 ± .02	
	$\therefore L = 1.23$	
	$\therefore L/D = 0.34$	
	$\therefore k = 0.75$	0.735
	Table I	Table II
	$\therefore s_{max} = f.\frac{kP}{LD}$	
	= 3.90 tons/sq. in.	3.82 tons/sq. in.
Specified shear stress	= 43 tons/sq. in.	25 tons/sq. in.
	$\therefore$ Reserve factor = > 10	6.5

It is not usual to record reserve factors greater than 10.

Note: 1. "Safety factor" usually implies a design requirement. "Reserve factor" indicates the reserve of strength over and above the design requirement.

2. These examples only cope with the threads. In general, for a complete stress analysis the screw in tension and torsion on the core area, the nut in bursting, etc., must also be considered.

7. CONCLUDING REMARKS

This paper is open to comment and criticism and such will be welcomed, as it would be very worthwhile to establish a reliable method of evaluating the strength of screw threads of Whitworth form.

More experimental data on the distribution of load along the engaged length of thread is called for.

APPENDIX

Determination of the stresses in a Whitworth thread.

Case under consideration:—
Diameter 2 inches.
T.p.i. 16.
Medium fit.
External thread.

Basic sizes for screw and nut:—
Maj. dia. 2.0000
Effec. dia. $2.0000 - 0.0400 = 1.9600$
Min. dia. $2.0000 - 0.0800 = 1.9200$
Tables from B.S. 84

Tolerance for screw and nut:—
Effec. dia. ± 0.0064
(Length of engag. 1 inch)

Worst limits for medium fit:—

Screw	Maj. dia.	1.9936
	Effec. dia.	1.9536
	Min. dia.	1.9136
Nut	Maj. dia.	2.0064
	Effec. dia.	1.9664
	Min. dia.	1.9264

Consider one turn of the thread.
Let the load be P.
Flank angle $\phi = 27.5°$.
Neglect the helix angle.
Neglect radial restraint.
Unit of length: inch.

1. *Surface SA.*

Mean dia. 1.9600
Width 0.0244
Area A_1 $0.150 \sec \phi$

$$\therefore \text{Bearing stress} = \frac{P \sec \phi}{A_1} = 6.7\,W$$

2. *Section AA', neutral axis.*

Dia. 1.9356
Depth 0.0406
Area A_2 0.247

Shear stress due to axial component:—

$$q = \frac{3}{2}\frac{P}{A_2}$$

Compressive stress due to radial component:—

$$p = \frac{P \tan \phi}{A_2}$$

$$\therefore \text{Max. shear stress} = \sqrt{[p^2/4 + q^2]} = 6.2\,P$$

$$\text{Principal stress} = p/2 + \sqrt{[p^2/4 + q^2]} = 7.2\,P$$

3. *Section BB', outer fibre.*

Dia. 1.9228
Depth 0.0473
Area A_3 0.286
Modulus z 0.00225
Offset e 0.0186
axial component
Offset f 0.0227
radial component to neutral axis

$$\text{Flexural stress:} — b = \frac{P}{z}[e - f \tan \phi]$$

$$\text{Compressive stress:} — c = \frac{W \tan \phi}{A_3}$$

$$\therefore \text{Max. direct stress} = x\,[b + c] = 7.3\,P$$

where x = stress concentration factor
= 1.5 (roundly)* Ref. 3.

For reserve factors, bearing and direct stresses are based on the proof stress of the material, shear stresses on the specified shear stress of the material. As the proof stress is at least 25 per cent. more than the specified shear stress (except for some non-ferrous metals with particularly low proofs) it is concluded that the max. shear stress at the neutral axis of section AA′ will be critical.

* Not to be confused with the stress concentration of axial tension.

As the effect of radial restraint affects only flexural stresses (and causes a relief), it may be neglected, the shear stress being critical.

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1. British Standard Specification for Screw Threads of Whitworth Form. B.S. No. 84-1940. British Standards Institution, May 1940.
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REVIEWS

The Business Law of Aviation.

G. O. Dykstra and L. G. Dykstra. McGraw Hill Book Company, Inc., New York and London, 1946. 30/- net.

The law on the surface of the earth is complicated enough in all conscience, and the increasing number of books on aviation law points the way to a three-dimensional complication which may prove very beneficial to the legal profession.

The book under review deals largely with American cases. They are selected for those situations which may confront the average person, be he an employee, a passenger or even a taxpayer interested in the wise expenditure of his money. Only one chapter is devoted to the International aspects of aerial law.

Although confined in the main to American cases there is much of interest and much of the unexpected to be found in these pages. For example, on page 491 appears the unexpected statement: " An aeroplane may be an instrument for committing the crime of manslaughter if it be so negligently operated." Actually one such verdict was returned, though reversed because the indictment was insufficient.

To quote again: " The aircraft, being structurally or aerodynamically unsafe or unstable, fell to the ground and Miss Bynum was killed. But because the indictment failed to aver that the infirmities in the aircraft were known to the defendant, it was defective."

This will cause many designers furiously to think !

An interesting feature of the book is the number of problems given at the end of each section for the reader to discuss. One example is worth quoting briefly. " Johnson hired a two-cockpit 'plane from Whitehead. Without a passenger in the rear cockpit the machine was nose heavy and Whitehead warned Johnson of this and not to fly from the front cockpit without a passenger. However, Johnson flew without a passenger in the rear cockpit, nose dived 35 feet from the ground, and damaged the aeroplane. Was he responsible to Whitehead for the damage ? "

The Story of the Air Training Corps.

Edited by Leonard Taylor. Air League of the British Empire, London, 1946. 3/- net.

The Air Training Corps came officially into existence on 1st February 1941. The roots of this remarkable and flourishing organisation were laid in July 1938, when the first Air Defence Cadet Corps Squadron was formed at Leicester. The growth of the Cadet Corps was so astonishing that nearly two hundred squadrons were formed in the country before the outbreak of war. The Corps was officially recognised and helped by the Air Ministry. Air Commodore (now Sir Adrian) Chamier, then Secretary General of the Air League, was its founder and subsequently first Commandant of the Air Training Corps, and the A.T.C., indeed the Royal Air Force itself, owes him an immense debt of gratitude for the drive and energy he gave to it.

From its official formation in February 1941 until V.J. day in 1945 nearly half a million young men had come to the A.T.C. and some 170,000 had completed their training and joined the fighting services. Hardly a single Honours and Awards list was issued without containing the name of one or more old A.T.C. cadets. Over 500 names were recorded in the lists of those decorated, including the V.C., D.S.O., D.S.C., D.F.C. and other awards, both British and Allied.

This story is a magnificent one of the history and work of a Corps to which this country owes so much. It is extremely well illustrated and produced.

The Aviation Mechanic's Engine Manual.

J. W. Vale, Jr. McGraw-Hill Book Company Inc., New York and London, 1946. 25/- net. 758 pp. Illustrated.

This book covers a wide field, from the fundamentals of aircraft engines to aircraft engine instruments for the engine mechanic, with an appendix on jet propulsion and atomic power.

Its object is to make a student familiar with the basic principles governing the construction and operation of modern aircraft engines and their accessories, and to outline briefly their inspection and maintenance procedure.

Although this book is written from the American point of view, with examples from American practice, it is one which is well worth studying by all those who intend to make the maintenance and overhaul of aircraft engines their *métier* in life. It is written partly in the descriptive manner, partly in the form of question and answer, the latter form being extremely well done.

The book is excellently reproduced.

It is a minor point of criticism, but the author's explanation, by means of an explosion in an open and closed sphere, for the working of a jet is misleading. Movement of the sphere does not depend upon a difference of pressure but on the fact that the centre of gravity of any system is not altered by internal forces of the system, and therefore is one part of the mass of the system is moved there must be a corresponding movement of other masses of the system to keep the position of the centre of gravity constant.

AN APPRECIATION

The Council of the Royal Aeronautical Society wish to thank those Companies who, by their generous co-operation, have done so much to help in the production of the Journal.

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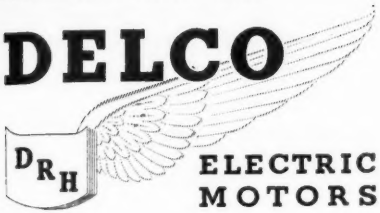

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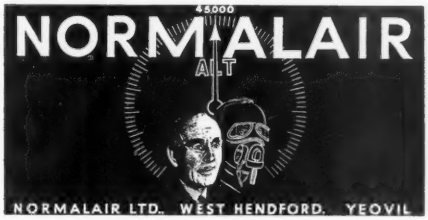

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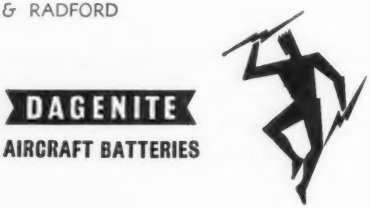

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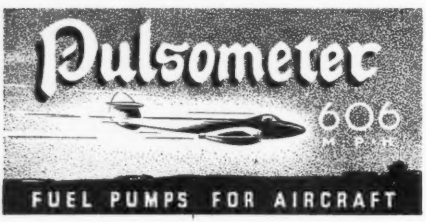

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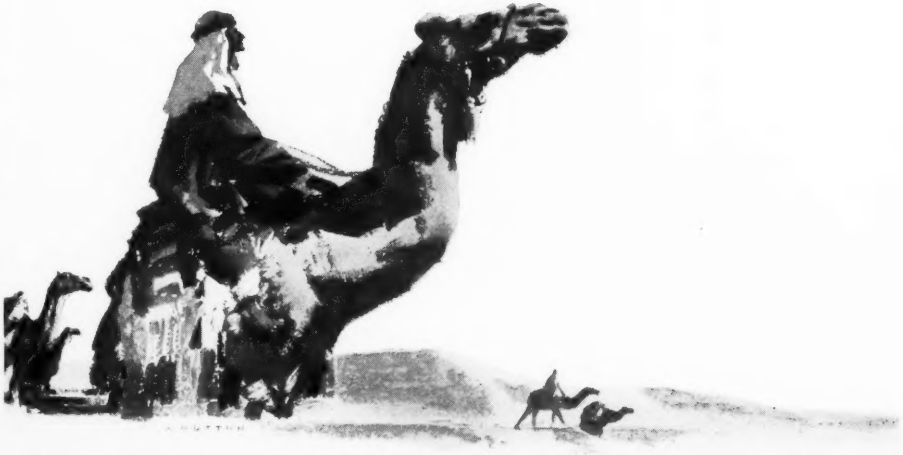

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